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PHILOSOPHICAL
TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCXCII.

PART I.

LONDON,

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MDCCXCII.

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ADVERTISEMENT.

The Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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APPENDIX.

Meteorological Journal kept at the Apartments of the Royal Society, by Order of the President and Council.

THE President and Council of the Royal Society adjudged, for the year 1791, two Medals on Sir Godfrey Copley's Donation; one to James Rennell, Esq. F.R.S. for his Paper on the Rate of Travelling as performed by Camels, printed in the last Volume of the Philosophical Transactions; the other to John Andrew de Luc, Esq. F.R.S. for his Improvements in Hygrometry.

ERRATUM.

P. 68, l. 23. *for* 2,1446 — 60,08711 *read* 2,14466 — 0,08711.

PHILOSOPHICAL

TRANSACTIONS.

I. *On the Ring of Saturn, and the Rotation of the fifth Satellite upon its Axis. By* William Herschel, *LL.D. F.R.S.*

Read December 15, 1791.

It is well known to Astronomers that the ring of Saturn becomes alternately enlightened on one of its sides, and that this change of illumination takes place when the planet passes through the node of the ring. This happened in October, 1789, when the southern plane, which had been in the dark for about fifteen years, became visible to us: an event to which I have looked forwards with considerable impatience. In the year 1790, the position of the ring was still too oblique to permit me to examine it well enough to form a proper judgment of its appearance, but lately I have been able to view it to greater advantage, with every one of my telescopes.

In a former paper,* where I ventured to hint at a division of the ring of Saturn, it was highly necessary to express that surmise with proper doubts concerning the reality of so wonderful a construction; but my late views of its southern plane, assisted by some conclusions drawn from the discovery of the quick rotation of the ring, have enabled me to speak decisively on this subject. My suspicion of a divided or double ring arose chiefly from the following circumstances.

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* Phil. Trans. Vol. LXXX. page 4.

In the first place, the black belt, during the time of about ten years that I observed it, on the northern plane, was subject to no kind of change; but remained always, permanently, of the same breadth and colour. With regard to its breadth, it is true that I could only judge of that part of it which goes across the body of the planet, by the rules of perspective, which made me suppose it to be as broad there as it was on the two sides; yet now, as we know that the ring revolves in about ten hours and a half, it is very certain, that the apparently narrow part, across the body, and that which was hidden behind the planet, in the course of an evening, when I have been observing Saturn for many hours together, must have been exposed to view in their full breadth, upon the sides of the ring; and that, if there had been any difference, I must have perceived it; especially as I was continually on the look out for such phænomena, by way of ascertaining, if possible, the rotation of the ring.*

In the next place, the colour of this dark belt was also uniformly the same, whenever I observed it under equally favourable circumstances; and being so well defined on both its borders, and, in every part of the revolving ring, presenting us with the same view of colour, breadth, and sharpness of its outlines, no kind of hypothesis but a division of the ring, through which the open heavens may be seen, will answer the conditions of this phænomenon. It remained therefore only to ascertain, whether the southern plane would present us with the same aspect. And since I have lately had a great number of fine views of the ring of Saturn, I shall here deliver as many of the observations as will be sufficient to throw light enough on the subject, to enable us to decide the question, whether this ring be double or single?

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* When I say that the black division was always of the same breadth, I do not mean to exclude very small variations, not only of the breadth of the black mark, but of the ring itself, which I have occasionally observed, and which it may be necessary, hereafter, to communicate at full length; but these, almost imperceptible, differences might arise from causes that are foreign to our present purpose.

Observations on the Ring of Saturn.

Sept. 7, 1790. 20-feet reflector. No dark division can as yet be seen upon the ring of Saturn; but it is hardly open enough to expect it to be visible.

Aug. 5, 1791. 20-feet reflector. The black list, on this side of the ring of Saturn, is exactly in the same relative place where I saw it on the northern plane.

Sept. 25, 1791. 20-feet reflector. The black division goes all around the ring, as far as I can trace it, exactly in the same place where I used to see it on the north side.

Oct. 13, 1791. 10-feet reflector. The black division upon the southern plane of Saturn's ring, is in the same place, of the same breadth, and at the same distance from the outer edge, that I have always seen it on the northern plane. With a power of 400, I see it very distinctly; it is of the same kind of colour as the space between the ring and the body, but not so dark.

Oct. 24, 1791. 7-feet reflector. With a new, machine-polished, most excellent speculum, I see that the division on the ring of Saturn, and the open spaces between the ring and the body, are equally dark, and of the same colour with the heavens about the planet.

20-feet reflector. The black division upon the ring is as dark as the heavens. It is equally broad on both sides of the ring. I see it very steadily, and can trace it a good way towards Saturn; both on the part of the ring which is turned towards us, and on that which lies the other way. I trace it as far as the place where a line, perpendicular to the direction of the ring, would touch the inside of the ring, or the outside of the open space between the ring and the body of the planet.

40-feet reflector. I see the division on the ring of Saturn of the same colour as the surrounding heavens. It is of an equal breadth on both sides, and I can trace it a great way towards the body of Saturn.

20-feet reflector. With a power of 600, I can trace the division very nearly as far as the place, where a perpendicular to the direction of the ring, would divide the open space between the planet and the ring, into two equal parts.

From these observations, added to what has been given in some former papers, I think myself authorized now to say, that the planet Saturn has two concentric rings, of unequal dimensions and breadth, situated in one plane, which is probably not much inclined to the equator of the planet. These rings are at a considerable distance from each other, the smallest being much less in diameter at the outside, than the largest is at the inside.

The dimension of the two rings and the intermediate space are nearly in the following proportion to each other.

Inside diameter of the smallest ring	5900 parts.
Outside diameter - - -	7510
Inside diameter of the largest ring	7740
Outside diameter - - -	8300
Breadth of the inner ring -	805
Breadth of the outer ring - -	280
Breadth of the vacant space -	115

Admitting, with M. DE LA LANDE, that the breadth of the whole ring, as formerly supposed to consist of one entire mass, is near one third of the diameter of Saturn, it follows that the vacant space between the two rings, according to the above statement, amounts to near 2513 miles.

In giving these proportions, which are merely taken from very accurate representations of the phænomena that offered themselves, I do not mean to be scrupulously exact, but reserve a greater accuracy for a future opportunity; when a micrometer, which I have lately applied to the 40-feet telescope, will assist me to have recourse to proper measures.

It may be remarked, that this opening in the ring must be of considerable service to the planet, in reducing the space that is eclipsed by the shadow of the ring to a much smaller compass; both on account of the direct light it lets through, and because there will be a strong reverberation of the rays of the sun between the two opposite edges. Moreover, if these rings should be surrounded by some atmosphere, which is highly probable, the refractions that will take place upon the edges will still contribute to lessen the darkness which the shadow of an undivided ring would have occasioned.

As we have now admitted Saturn to have two rings entirely detached from each other, so as plainly to permit us to see the open heavens through the vacancy between them; and as in my former paper I have given the revolution of the ring, which was then supposed to be all in one united mass, it will be necessary to examine, whether both rings partake in the same revolution, or to which the period which has been assigned belongs?

To decide this point, we must recur to the observations of the spots by which the rotation of the ring was determined. The spot called α,* for instance, which has been observed to revolve with great regularity through upwards of 300 periods, between the 28th of July and the 24th of December, 1789, was certainly situated pretty near the outer edge. The spot β, as may be gathered from the observation of the 16th of September, and 25th of December, was most likely on the very edge itself; nor could the spot δ be far from it. This, without considering the situation of γ and ε, is quite sufficient to determine us to assign the period we have given to belong to the large, thin and narrow, outward ring.

The spots γ and ε were probably at some distance from the outward edge of the outer ring; but this distance might possibly not exceed that of the inside edge of the same ring. We may however admit them to have adhered to the inner ring, whose rotation is perhaps not very different from that of the outer one; or we may examine whether these two spots may not perhaps agree to some other supposed revolution of the inner ring; but then the observations that are given of them will hardly be sufficient for establishing the time of that ring's rotation with accuracy, though they undoubtedly must amount to a proof that it also revolves with great volocity on its axis.

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* See Phil. Trans. Vol. LXXX. page 481.

That there should be a small difference in the periods of the rotation of the two rings, is highly probable from their different dimensions; and now, that the rotation is known, the division of it into two parts seems to be a very natural consequence of its construction. For, when the extreme thinness is taken into consideration, we find by KEPLER's law, of the periods of revolving bodies placed at different distances, that it would be very wonderful for so thin, and so broad a plane, to have adhesion enough to keep together; and that consequently this ring in its divided state, supposing the rotation of the parts to favour the construction, is more permanent than it would be otherwise. This however is only mentioned as a collateral circumstance, and by no means intended either as a proof of the division, or the different rotation of the two parts of the ring. For, notwithstanding we cannot but set the highest value upon the excellent theories that have been lately delivered in the Memoirs of a learned Society, of which I also have the honour to be a member, we must refer entirely to observation for the necessary data on which to found our subsequent computations.

The memoir to which I allude * refers to observations of many divisions of the ring of Saturn. This must lead us to consider the question, whether the construction of this ring is of a nature so as permanently to remain in its present state? or whether it be liable to continual and frequent changes, in such a manner as in the course of not many years, to be seen subdivided into narrow slips, and then again as united into one or two circular planes only? Now, without entering into a discussion, the mind seems to revolt, even at first sight,

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* See *Histoire de l'Académie Royale des Sciences de Paris,* 1787, page 249.

against an idea of the chaotic state in which so large a mass as the ring of Saturn must needs be, if phænomena like these can be admitted. Nor ought we to indulge a suspicion of this being a reality, unless repeated and well-confirmed observations had proved, beyond a doubt, that this ring was actually in so fluctuating a condition. Let us therefore examine what facts we have to guide us in this inquiry.

After looking over all my observations upon Saturn, since the year 1774 to the present time, I can find only four where any other black division upon the ring is mentioned than the one which I have constantly observed, and from which I have deduced the actual division of the ring into two very unequal portions. These observations are as follows.

June 19, 1780. 10^h 15′ mean time. With a new 7-feet speculum, having an aperture of 6,4 inches, with also a much improved small speculum, and a power of about 200. I see a second black list upon the ring of Saturn, close to the inner side, on the preceding arm of the ring. See figure 1. Tab. I.

June 20, 1780. 10^h 10′. I see the same double list on the preceding side of the ring.

June 21, 1780. 10^h 1′. Small 20-feet, Newtonian reflector, power 200. I see the second black list on Saturn's ring. It is closer to the inside than the other is to the outside; but it is only visible on the preceding side of the ring. See figure 2.

June 26, 1780. 9^h 34′. Small 20-feet, Newtonian reflector; aperture confined to 7 inches. The 2d black list, on the preceding side of the ring of Saturn, is visible.

June 29, 1780. 10^h 19′. Saturn's belts are very clear. I see but one black list upon the ring. The shadow of the planet is visible upon the side of the ring, as well as upon the small northern part that projects beyond the planet. See fig. 3.

Nov. 21, 1791. 0^h 28′ sid. time. 40-feet reflector, power 370. There is no other black division visible upon the ring of Saturn but the one near the outer edge.

It must be confessed that Saturn was in the very best situation for viewing the plane of the ring, when the first four observations were made; and that consequently they may be looked upon as a strong evidence for another division. But hitherto I have set them aside as wanting more confirmation, not only because I could never perceive the same dark line on the following side of the ring as well as on the preceding side; nor since I could not find it on the 29th of June, 1780, as we have seen above; but chiefly, because I have not been able, with any of my best instruments, to see it again at all. We also find by the observation of the 21st of November, 1791, which has been added, that the southern plane, as yet, presents us with no other division than the capital one, which I have observed these thirteen years, on both sides of the ring. However, if the opening should be very narrow, and the rings eccentric, it is possible that a dark line might by this means become visible on one side only. Moreover, these objects may be so minute, that no other time than when the plane of the ring is exposed as much as it can possibly be, will do to ascertain such phænomena. This will happen again about the year 1796, when we may hope to have a satisfactory view of it, with our large instruments.

It remains now to consider the observations that have been made by M. Cassini, Mr. Short, and Mr. Hadley.

Without being in possession of the original observations of

M. CASSINI, I cannot decide whether the black list which he saw was the same which I have observed. M. DE LA LANDE says (*Ast.* Vol. III. page 441.) that CASSINI saw it divided by a small black line into two *equal* parts. M. DE LA PLACE (*Mémoire sur la Théorie de l'Anneau de Saturne*) mentions that CASSINI saw the breadth of the ring divided into two parts *almost* equal. It should seem from this, that M. CASSINI was not particularly attentive to the proportions of the division; in which case his observations and mine will agree perfectly well; but if he has any where expressly mentioned, that the ring was divided into *equal* parts, so that we may be certain he was particularly attentive to that circumstance, it will follow evidently that the ring, since his time, has undergone a very capital change in its construction.

Mr. SHORT assured M. DE LA LANDE, that he had seen many divisions upon the ring, with his telescope of 12-feet. A thing of such consequence, and so new, ought certainly to have been given in a more satisfactory and circumstantial way than only by communicating it, from memory, in conversation, to another person. Besides, it is well known that many telescopes will give double and treble images, and that especially those which have large apertures are subject to tremors, which multiply small lines. For these reasons, we can hardly take into account observations that seem not to be sufficiently established. What has been said is, however, by no means intended to undervalue Mr. SHORT's observations; and this, I hope, will be evident, when it is remembered how scrupulously I have just before set aside four of my own, because I looked upon them as not sufficiently confirmed.

Mr. HADLEY's observation of the division of the ring, with a

$5\frac{1}{2}$-feet Newtonian reflector, which was certainly a very excellent instrument, agrees perfectly well with mine.

From what has been said, it does not appear to me that there is a sufficient ground for admitting the ring of Saturn to be of a very changeable nature, and I guess that its phænomena will hereafter be so fully explained, as to reconcile all observations. In the mean while, we must withhold a final judgment of its construction, till we can have more observations. Its division, however, into two very unequal parts, can admit of no doubt; and the following are measures taken of the diameter of the largest or outward ring.

Oct. 7, 1791. *Correction of the 20-feet clock* — 2′ 16″,5.

Measures of the ring of Saturn with the 20-feet reflector.

0h 37′	1st measure	54″,115
	2d ———	52 ,537
	3d ———	52 ,875
	4th ———	54 ,679
	5th ———	52 ,903
	6th ———	53 ,044
	7th ———	53 ,411

53″,366 mean of the seven measures.

Proper allowance was made for the wires being tangents to the outside of the ring.

When this measure is reduced to what it would be at the mean distance of Saturn from the earth, we have 46″,832.

Oct. 24, 1791. *Correction of the 40-feet clock* + 25″,4.

Measure of the ring of Saturn with the 40-feet reflector. Power 370.

1h 3′	1st measure	53″,914
	2d ———	53 ,260
		53″,587 mean of the two measures.

Reduced to the mean distance of Saturn, the measure is 47″,241.

Nov. 21, 1791. *Correction of the 40-feet clock* — 7″,8.

Another measure of the ring of Saturn with the 40-feet reflector. Power 370.

0h 48′	1st measure	50″,627
	2d ———	50 ,042
	3d ———	50 ,808
		50″,492 mean of the three measures.

Reduced to the mean distance of Saturn the measure is 45″,803.

Oct. 24	47″,241
Nov. 21	45 ,803
	46″,522 mean of the measures with the 40-feet reflector.

40-feet	46″,522
20-feet	46 ,832
	46″,677 mean of all the measures.

By way of forming more easily a comparative idea of the stupendous size of this ring of Saturn, I have calculated the proportion it bears to the earth, and find that its diameter is to that of the latter as 25,8914 to 1; and that consequently, when seen at the mean distance of the sun, it will subtend an angle of 7′ 25″,332.

From the above proportions we also compute that this ring must be upwards of 204883 miles in diameter.*

On the Rotation of the fifth Satellite of Saturn, on its Axis.

In my frequent observations of the Saturnian system, I remarked, that the 5th satellite is subject to a change of brightness. This having been noticed before by other observers, I did not at first pay so much attention to it as I soon afterwards found this circumstance deserved. When I saw this satellite always assume the same brightness in the same part of its orbit, and perceived that its change was regular and periodical, it occurred to me very naturally, that the cause of this phænomenon could be no other than a rotation upon its axis. It became necessary therefore to find out a method to determine the time of this rotation.

In order to investigate this, I pursued the satellite with great attention, and marked all its changes of apparent brightness. The result of many observations is as follows. The light of the satellite is in full splendour during the time it runs through that part of its orbit which is between 68 and 129 degrees past the inferior conjunction. In passing through this arch it does not fall above one magnitude short of the brightness of the 4th satellite. On the contrary, from about 7 degrees past the opposition till towards the inferior conjunction, it is not only less bright than the 3d, but hardly, if at all, exceeds the 2d, or even the 1st satellite; provided the latter be then about its greatest elongation, where its light is least impeded by the brightness of the planet. Upon the whole, the alteration seems to amount to what among the fixed stars, and with the naked eye, would be called a change from the 5th to the 2d, and from the 2d to the 5th magnitude.

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* In this calculation I have used for the earth's diameter the sum of the longer and shorter semi-axes which are given in Mr. Dalby's Paper, published in the last volume of the Philosophical Transactions. If we compute the vacant space between the two rings immediately from the above dimensions of the outward ring, we shall have 2839 miles; and this will certainly be more accurate than the result which has been drawn from the proportion of the breadth of the ring to the diameter of Saturn.

Having thus observed this satellite, for many of its revolutions round the primary planet, to lose and regain its light regularly, it is evident that the time of its rotation on its axis cannot differ much from that of its revolution round Saturn. I think myself sufficiently authorized to make this conclusion, notwithstanding it may have happened sometimes that the light of the satellite has suffered an occasional change, of short duration, from other causes; for the same reason that we should certainly allow those who first saw the spots in the sun to be in the right to assign the period of its rotation *nearly*, when they perceived that the same spot made several revolutions, notwithstanding that spot might afterwards vanish. But I may go farther, and ascertain upon sufficient grounds, that this satellite turns once upon its axis, exactly in the time it performs one revolution round its primary planet. This degree of accuracy is obtained by taking in the observations of M. CASSINI, which are related in the *Mémoires de l'Académie des Sciences*, 1705, page 121; where we find it mentioned, that "the 5th satellite of Saturn disappears regularly for about one "half of its revolution, when it is to the east of Saturn." The same memoir contains also a conjecture of this satellite's rotation upon its axis; but this surmise is contradicted as premature, in 1707, page 96; where we find the following paragraph. " M. Cassini gives an example of the danger there is " in these sort of determinations, that are made too hastily. " The 5th satellite of Saturn, of which we have said, in the " History of 1705, page 121, that it grew invisible, in the " eastern half of the circle it describes about Saturn, began, " in the month of Sept. 1705, to be there visible, as well as in " the western half, where it always was so. Hence the con- " jectures which we have related cease to be well founded."

Now, without determining whether the satellite, from some cause or other, ceased to change its brightness, or whether its phænomena were not sufficiently followed to come to a proper conclusion, I think that with the assistance of observations at so great a distance of time as those of M. Cassini, I may sufficiently establish the period of this satellite's rotation. For since I have traced the regular, and periodical change of light, through more than ten revolutions, and find them, in all appearance, to be contemporary with its return about Saturn, it leads us directly to a strong presumption that its rotation upon its axis, like that of our moon, strictly coincides with its revolution round its primary planet; and the observations of M. Cassini completely confirm this conclusion. For, had he seen the satellite brightest in any other part of its orbit, our observations would not have agreed together; but since the year 1705, the satellite has made about 397 revolutions; and yet the phænomena described by Cassini answer now as exactly to my own observations, as the spots in our moon, viewed in Cassini's time, answer to those we now observe.

If it should be objected, that the 5th satellite of Saturn has not been continually observed, and that consequently these appearances might either not happen at all, or fall upon different places in its orbit; I answer, that a period of more than ten revolutions, which I have included, is already a strong argument that no such change has taken place; for if the satellite had but made a single rotation upon its axis more or less than it has made revolutions round Saturn, the change must amount to nearly one degree *per* revolution; that is, to about ten degrees during the time of my taking notice of it; which is a quantity I think I might have perceived. However, to remove all doubt, we have some valuable observations of M. BERNARD, who in the year 1787, also found the 5th satellite of Saturn subject to the same change of light that M. CASSINI had observed.* Now, by joining those to mine, we have a short period of near 20 revolutions that agree together, so as to preclude all doubt of any intermediate change; and therefore we cannot be liable to err, when we extend this period to all the 397 revolutions since CASSINI's time, and by that means ascertain that the 5th satellite of Saturn turns upon its axis, once in 79 days, 7 hours, and 47 minutes.

I cannot help reflecting, with some pleasure, on the discovery of an analogy, which shews that a certain, uniform plan is carried on among the secondaries of our solar system; and we may conjecture, that probably most of the moons of all the planets are governed by the same law; especially if it be founded on such a construction of the figure of the secondaries, as makes them more ponderous towards their primary planets. For, if even the 5th satellite of Saturn, which is at so great a distance from its planet, is affected by such a law, of course the other satellites are not very likely to have escaped its influence.

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* See *Mémoires de l'Académie,* 1786, page 378.

From the considerable change in the brightness of the 5th satellite of Saturn, we may be certain that some part of its surface, and this by far the largest, reflects much less light than the rest; and, from the points of its orbit in which it appears brightest to us, we conclude that neither the darkest nor brightest side of the satellite is turned towards the planet, but partly one and partly the other; though probably rather less of the bright side.

The great regularity of this change of brightness seems to point out another resemblance of this satellite with our moon. It is well known that we see the spots of the moon pretty nearly of the same brightness, so as not to be overcast in a very strong degree by dense clouds to disfigure them, and therefore have great reason to surmise that her atmosphere is extremely rare; which indeed we also know from other principles: In like manner, on account of the uninterrupted changes in the brightness of the 5th satellite of Saturn, we may suppose that it also partakes of a similar fate with respect to its atmosphere, which is probably as rare as that of our moon.

On the Distance of the fifth Satellite.

The distance of the 5th satellite from Saturn is allowed to be the most proper for obtaining a true measure of the quantity of matter contained in the planet; for which reason I have taken many measures of it with the 20-feet reflector. I give them at full length, that the validity of them may appear in its proper light.

Sept. 25, 1791. *Correction of the clock* — 2′ 19″,5 *for midnight.*

Distance of the 5th satellite of Saturn from the centre of the planet; measured with the 20-feet reflector, and a magnifying power of 157.

23^{h} 4′	1st measure	8′ 55″,684
23 19	2d ———	8 53 ,175
23 33	3d ———	8 59 ,179
23 47	4th ———	8 52 ,123
23 55	5th ———	8 56 ,361
0 2	6th ———	8 55 ,797

8′ 55″,5 mean of the six measures.

Sept. 26, 1791. *Correction of the clock* — 2′ 19″,8.

23^{h} 15′	1st measure	9′ 3″,745
23 25	2d ———	9 2 ,758
23 31	3d ———	9 7 ,014
23 38	4th ———	9 6 ,592
23 42	5th ———	9 8 ,001
23 45	6th ———	9 6 ,479

9′ 5″,8 mean of the six measures.

Sept. 27, 1791. *Correction of the clock* — 2′ 20″,0.

22^{h} 53′	1st measure	9′ 20″,656
22 57	2d ———	9 20 ,359
23 1	3d ———	9 20 ,149
23 5	4th ———	9 20 ,641
23 11	5th ———	9 20 ,064
23 16	6th ———	9 20 ,840

9′ 20″,5 mean of the six measures.

Sept. 28, 1791. *Correction of the clock* — 2′ 20″,2.

21^{h} 38′	1st measure	9′ 27″,759
21 44	2d ———	9 29 ,957
21 46	3d ———	9 27 ,815
21 50	4th ———	9 28 ,238
21 53	5th ———	9 28 ,970
21 58	6th ———	9 27 ,420

9′ 28″,4 mean of the six measures.

Sept. 29, 1791. *Correction of the clock* — 2′ 20″,4.

0^{h} 17′	1st measure	9′ 37″,060
0 22	2d ———	9 36 ,552
0 26	3d ———	9 36 ,270
0 29	4th ———	9 36 ,862
0 33	5th ———	9 37 ,765
0 36	6th ———	9 37 ,060

9′ 36″,9 mean of the six measures.

Sept. 30, 1791. *Correction of the clock* — 2′ 20″,6.

20^{h} 25′	1st measure	9′ 39″,258
20 29	2d ———	9 38 ,441
20 34	3d ———	9 37 ,596
20 38	4th ———	9 37 ,032
20 40	5th ———	9 40 ,949
20 42	6th ———	9 37 ,793

9′ 38″,5 mean of the six measures.

Supposing the satellite now to be not far from its greatest elongation, I measured the declination between the centre of Saturn, and the 5th satellite; causing one to pass along one wire, while the other followed upon the other wire.

22^h 32′	1st measure	1′ 41″,889
22 47	2d ———	1 45 ,609

1′ 43″,749 mean of the two measures.

Not being satisfied with the considerable disagreement, I took another measure with the utmost precaution and care; as the apparent curvature of the wires at so great a distance, required more than common attention.

0^h 52′ very exact. 1′ 43″,354

Mean between this and the former mean, 1′ 43″,55 south of the parallel of Saturn.

The satellite not being perhaps arrived at its greatest elongation, I took six other measures of its distance.

1^h 5′	1st measure	9′ 41″,907
1 11	2d ———	9 38 ,723
1 14	3d ———	9 38 ,159
1 18	4th ———	9 41 ,203
1 23	5th ———	9 40 ,385
1 26	6th ———	9 41 ,935

9′ 40′,4 mean of the six measures.

In the last six measures of the 5th satellite, I used a method a very little different from that which I employed before, and which is probably more accurate. I used to observe, when the two wires were nearly brought to their proper distance, the moment of intersection of the satellite ; and the instant it was hid behind the wire, cast my eye on Saturn, which should be bisected when the measure is justly taken. But this change of attention cannot be made without some very small loss of time. To correct this defect I took alternately the bisection of Saturn, and cast my eye upon the satellite ; and the bisection of the satellite, casting the eye upon Saturn. As the latter way gives the interval too small, the former gives it too large, and between both the true measure may be obtained. I do not, however, suppose, that the error of the former method can amount to so much as a single second of space ; as, knowing the loss of time, I always used the utmost precaution ; and repeated the examination of a measure perhaps 20 times before I let it pass.

Oct. 1, 1791. *Correction of the clock* — 2′ 20″,0.

22ʰ 25′	1st measure	9′ 43″,767
22 27	2d ———	9 44 ,444
22 30	3d ———	9 43 ,007
22 32	4th ———	9 42 ,499
22 34	5th ———	9 40 ,554
22 36	6th ———	9 43 ,965

9′ 43″,0 mean of the six measures.

It grew cloudy, so that no measures later in the night could be obtained ; nor could I get another sight of Saturn till October the 7th, when the satellite was far advanced in its orbit, on its return towards the planet.

Supposing the satellite to have been very nearly at its greatest elongation, when the last six measures were taken, I have reduced them to the mean distance of Saturn, where they give 8′ 31″,97.

I forbear making deductions from this result, with respect to the quantity of matter contained in the planet, as, possibly, the orbit of the satellite may be considerably elliptical; in which case measures taken in opposite parts of that orbit will be required, before we can make a strict application of the laws of centripetal forces.

Philos. Trans. MDCCXCII. *Tab.* I. *p.* 22.

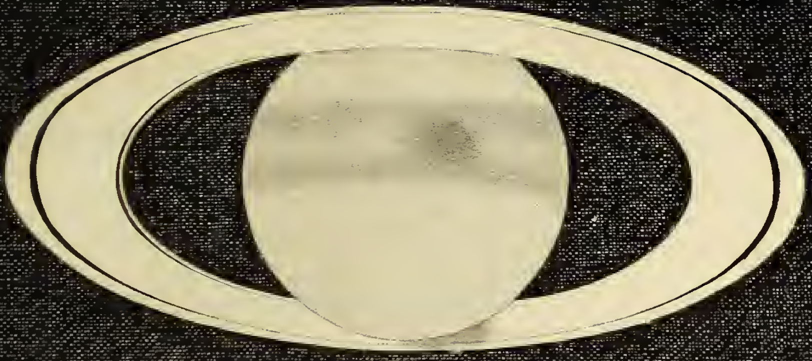

Fig 1

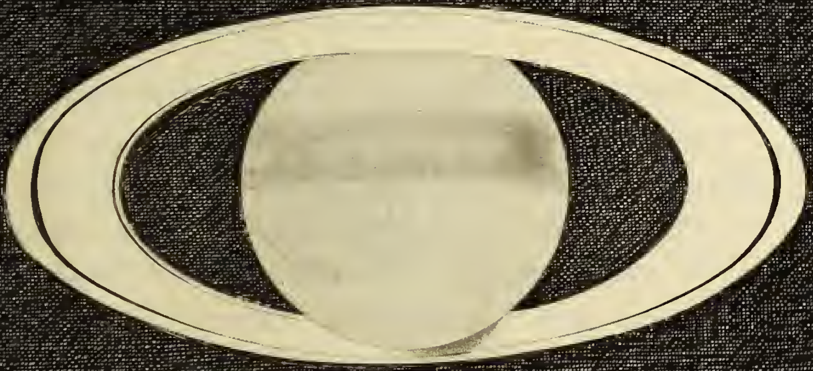

Fig 2

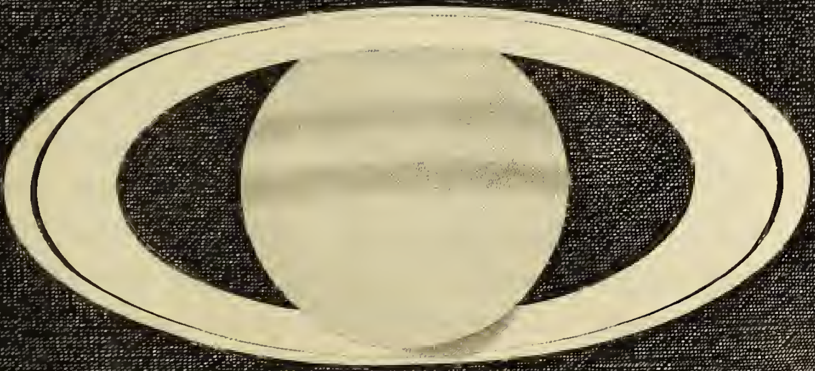

Fig 3

Basire sc.

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NATURAL HISTORY.

II. *Miscellaneous Observations.* *By* William Herschel, *LL.D. F.R.S.*

Read December 22, 1791.

Account of a Comet.

Last Thursday evening, the 15th of December, about half after eight o'clock, while I was taken up with observing Saturn, my sister looked over the heavens, and discovered a pretty large, telescopic comet, in the breast of Lacerta. I viewed it in my seven-feet reflector, and with that instrument settled its place and rate of moving. At $9^h\ 42'\ 4'',8$ true mean time, it preceded a small telescopic star $11'',3$ in time, and was $2'\ 41''$ south of the same. The place of this star I have since determined with sufficient accuracy, that it may be found again by those who wish to settle it more exactly. It follows the 2d of Flamsteed's stars in the constellation of Lacerta, $1'\ 41'',5$ in time; and is $45'\ 40'',8$ more south than the same. The apparent motion of the comet on Thursday evening was direct, and at the rate of about three minutes of time in right ascension, and a little more than two degrees in polar distance *per* day; from which we may suppose that we shall keep it some time in view. Last night I examined it with a twenty-feet reflector, and found it to consist of a great light, pretty regularly scattered about a condensed small part of five or six seconds in diameter; which resembled a kind of nucleus, but had not the least appearance of a solid body. Beside the scattered, and gradually diminishing light, which reached nearly to a distance of three minutes every way beyond the bright centre, there was also a faintly extended, ill defined, pretty broad ray, of about 15 minutes in length, directed towards the north following part of the heaven, which might be called the tail of the comet.

Its place for the same night (Dec. 16th) was determined by a five-feet Newtonian *Sweeper*, carrying an equilateral triangle in the focus of the eye-glass, not so large but that the three intersections, made by the wires at the three angles, may be distinctly perceived. At $5^h\ 49'\ 40'',6$ it preceded the 6th Lacerta $4'\ 58'',5$ in time, and was $52'\ 14'',5$ more north than that star.

On the periodical Appearance of o Ceti.

The changeable star in the neck of the Whale, *o* Ceti, continues its variations as usual, but with some considerable irregularities of brightness.

In the year 1779, as we have seen*, it excelled α Arietis so far as almost to rival Aldebaran; and continued in that state a full month.

In 1780, its greatest brightness was only like that of δ Ceti.

In the year 1781, it did not come up to the brightness of δ.

In 1782, this star increased to the size of β Ceti, and continued bright for more than twenty days.

In 1783, it did not only vanish to the naked eye, as usual, but disappeared so completely, that I could not find it with a telescope, which permitted not a star of the 10th magnitude to escape me. When it increased again, it did not amount to the brightness of δ.

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* Phil. Trans. Vol. LXX. page 338.

In 1784, I saw it only of the 8th magnitude in a twenty-feet reflector, but as I did not continue to observe it regularly, it might possibly change as usual.

In 1789, it arrived to the brightness of α Piscium, or rather excelled it.

In 1790, the greatest brightness was almost equal to that of α Ceti.

In the present year, I have seen it only of the magnitude of γ Ceti nearly; or between γ and δ; but, as bad weather has occasioned many interruptions, it may possibly have been larger.

The period of 333 days, assigned by BOUILLAUD, does not agree with present observations compared to those of FABRICIUS made on the 13th of August, 1596, when this star was in its greatest lustre. M. CASSINI also found, that his observations, in the beginning of August, 1703, when the star was brightest, did not agree with the interval of 333 days; and therefore, supposing the star to have changed 117 times since the epoch of FABRICIUS, he gave it a period of 334 days. This will, however, not agree with the present time of the changes; and it appears now that M. CASSINI ought to have assumed 118 instead of 117 variations; which would have pointed out a period of 331 days, and some hours.

That this is, probably, very near the real time of the star's variation, will be seen when we admit it to have undergone 214 changes between the 13th of August, 1596, and the 21st of October, 1790; by which long interval we obtain the period of 331 days, 10 hours, 19 minutes. It will, indeed, be necessary, in order to reconcile all observations, to admit of some occasional deviations in the appearance of the star, amounting almost to a month; but that this is no more than we may allow, is pretty evident from the variations I have taken notice of within the last 14 years; besides, a period of 334 days could not be admitted without totally giving up all regularity in the returning appearance of the star.

I have taken the epoch of the 21st of October, 1790, as one of the best ascertained, moderate appearances I have been able to obtain; and believe it to be more proper for settling the period, than that which might be deduced from a brilliant blaze of the star, such as took place in 1779, owing to causes that are not regular, and therefore may be apprehended to disturb the general order of the change.

On the Disappearance of the 55th Herculis.

Among the changes that happen in the sidereal heavens we enumerate the loss of stars; but, notwithstanding the real destruction of an heavenly body may not be impossible, we have some reasons to think that the disappearance of a star is probably owing to causes which are of the same nature with those that act upon periodical stars, when they occasion their temporary occultations. This subject, however, being of great extent and consequence, we shall not enter into it at present, but only relate a recent instance of the kind.

Two stars of the 5th magnitude, whose places we find inserted in all our best catalogues, were to be seen in the neck of Hercules. They are the 54th and 55th of FLAMSTEED'S, in that constellation. In the year 1781, the 10th of October, I examined them both, and marked down their colour, *red*. The 11th of April, 1782, I looked at them again, and noted my having seen them distinctly, with a power of 460; and that they were single stars.

The 24th of last May, I missed one of the two, and examining the spot again the 25th, and many times afterwards, found that one of them was not to be seen. The situation of the stars is such that, not having fixed instruments, I could not well determine which of the two was the lost one. I therefore requested the favour of my much esteemed friend, the astronomer royal, to ascertain the remaining star; and it appears from Dr. MASKELYNE'S answer to my letter, that the 55th Herculis is the one which we have lost.

Remarkable Phænomena in an Eclipse of the Moon.

The 22d of October, 1790, when the moon was totally eclipsed, I viewed the disk of it with a twenty-feet reflector, carrying a magnifying power of 360. In several parts of it I perceived many bright, red, luminous points. Most of them were small and round. The brightness of the moon, notwithstanding the great defalcation of light occasioned by the eclipse, would not permit me to view it long enough to take the places of these points. They were, indeed, very numerous; as I suppose that I saw, at least, one hundred and fifty of them. Their light did not much exceed that of Mons Porphyrites HEVELII.

We know too little of the surface of the moon to venture at a surmise of the cause from whence the great brightness, similarity, and remarkable colour of these points could arise.

Slough, Dec. 17, 1791.

III. *Experiments and Observations on the Production of Light from different Bodies, by Heat and by Attrition. By Mr.* Thomas Wedgwood; *communicated by Sir* Joseph Banks, *Bart. P. R. S.*

Read December 22, 1791.

Before I begin to state the experiments which are the subject of this Paper, it may not perhaps be improper to give a very compendious history of the discoveries which have already been made relative to phosphoric bodies; omitting, however, the electrical phosphori, and such as are evidently consumed or decomposed in the emission of their light, as these are well known, and are too numerous and important to be slightly noticed.

Pliny was well acquainted with the luminous appearance of rotten wood, and of the eyes of dead fish. From this time I find nothing relative to the phosphorism of bodies, till the beginning of the sixteenth century, when Benvenuto Cellini, in his Art of Jewellery, mentions his having seen a carbuncle shine in the dark like coals nearly burnt out; and relates a story of a coloured carbuncle having been found in a vineyard near Rome, by its shining in the night. About the year 1639, Vincenzo Cascariolo, of Bologna, discovered, by accident, that when a certain stone found in that neighbourhood was calcined in a particular manner, it acquired the remarkable property of absorbing the light of the sun, of retaining it for some time, and of emitting it in the dark: subsequent experimenters found it to do the same with the light of a candle. In 1663, Mr. BOYLE observed a particular diamond to give out a light almost equal to that of a glow-worm, when heated, rubbed, or pressed; and investigated very fully the nature of the light of dead fish, flesh meat, and rotten wood. In 1677, BALDWIN of Misnia discovered, in the residuum of a distillation of chalk and nitrous acid, a phosphorus similar in its properties to the Bolognian, but not possessing the phosphoric virtue in so eminent a degree. In 1705, Mr. FRANCIS HAWKESBEE found that glass rubbed on glass, in common air, in the vacuum of an air-pump, or under water, "exhibited a considerable light." In 1724, M. DU FAY discovered that almost all substances which could be reduced to a calx by fire only, or after solution in the nitrous acid, absorbed and emitted light like the phosphorus of CASCARIOLO and of BALDWIN; and that some diamonds, emeralds, and many other precious stones emitted light in the dark, after being exposed to the rays of the sun. About the same time, BECCARIA of Turin found almost every body in nature to be luminous after a similar exposure: he added, too, this very important discovery; that an artificial phosphorus, exposed to the light in a coloured glass vial, emits, in the dark, rays of the identical colour of the vial. Mr. MARGRAAF, by an analysis of the Bolognian stone, shews that it contains vitriolic acid united to calcareous earth, and that all gypseous stones treated like the Bolognian, provided they are pure from iron, become phosphorescent. About the year 1764, Mr. CANTON made a phosphorus of sulphur and oyster-shells calcined together, and distinguished himself by many curious experiments made with it; he found that his phosphorus might be made to shine by heating it, after it had ceased to be luminous of itself, but that the same heat would have this effect for a certain time only. Heat has been observed by several of these philosophers to promote the emission, and to shorten the duration, of the light of phosphori. Fluor has been long known to give a fine bright light when heated. D. HOFFMAN discovered that red blende and feldspat were luminous when pieces of either were rubbed together. POTT extended this discovery to all pure flints and crystals, and to porcelain. KEYSLER found glacies mariæ to be luminous when heated. M. DE LA METHERIE has observed some neutral salts and calcareous earths to be luminous in the same way. The Count de RAZOUMOWSKI, in a Memoir of the Physical Society of Lausanne, shews that quartz and glass give out light, when struck by almost any hard body, and that some few other bodies are luminous, when pieces of the same kind are rubbed upon one another; he finds quartz to give out its light under water.

This brief account includes, as far as I am able to collect, the chief discoveries which have been made concerning luminous bodies. I was led to make the following experiments from observing the light which proceeds from two quartz pebbles rubbed against each other: I searched for this property in many other bodies with success, but met with two soft stones, which did not afford any light upon the most violent attrition. Conceiving that heat might probably be the cause of the light emitted by quartz from attrition, I attributed this failure to a want of sufficient hardness in these friable stones for producing the necessary heat. Accordingly, sprinkling some of their powder on a plate of iron nearly red hot, I had the satisfaction to observe it emitting a considerable light. Extending this mode of trial, I found that the phosphorism of almost all bodies might be made apparent either by heat or by attrition; I shall therefore divide the subject of this Paper into two parts: I. On the light produced by Heat.—II. On the light produced by Attrition.

I.

The best general method of producing the light by heat is, to reduce the body to a moderately fine powder, and to sprinkle it, by small portions at a time, on a thick plate of iron, or mass of burnt luting made of sand and clay, heated just below visible redness, and removed into a perfectly dark place.

The following is a list of such bodies as I have found to be luminous by this treatment, arranged according to the apparent intensity of their light.

1. Blue fluor, from Derbyshire, giving out a fetid smell on attrition.
2. Black and grey marbles, and fetid white marbles, from Derbyshire.
Common blue fluor, from Derbyshire.
Red feldspat, from Saxony.
3. Diamond.
Oriental ruby.
Aerated barytes, from Chorley, in Lancashire.
Common whiting.
Iceland spar.
Sea shells.
Moorstone, from Cornwall.

White fluor, from Derbyshire.
4. Pure calcareous earth, precipitated from an acid solution.
—— argillaceous earth (of alum).
—— siliceous earth.
—— new earth, from Sydney Cove.
Common magnesia.
Vitriolated barytes, from Scotland.
Steatites, from Cornwall.
Alabaster.
Porcelain clay of Cornwall.
Mother of pearl.
Black flint.
Hard white marble.
Rock crystal, from the East Indies.
White quartz.
Porcelain.
Common earthen ware.
Whinstone.
Emery.
Coal ashes.
Sea sand.
5. Gold, platina, silver, copper, iron, lead, tin, bismuth, cobalt, zink.
Precipitates by an alkali from acid solutions of gold, silver, copper, iron, zink, bismuth, tin, lead, cobalt, mercury, antimony, manganese.
Vitriolated tartar,
Crystals of tartar,
Borax,
Alum,
previously exsiccated.

Sea coal.
White paper,
—— linen, } in small pieces.
—— woollen,
—— hair powder.
Deal sawdust.
Rotten wood (not otherwise luminous).
White asbestos.
Red irony mica.
Deep red porcelain.
6. Antimony, nickel.
Oils, lamp, linseed, and olive,
White wax, } luminous at and below boiling.
Spermaceti,
Butter,

The duration of the light thus produced from different bodies is very unequal; in some the light is almost momentary, in others it lasts for some minutes, and may be prolonged by stirring the powder on the heater. It soon attains its greatest brightness, and dies away gradually from that point, never appearing in a sudden flash, like the light of quartz pebbles rubbed together. If blown upon, it is suddenly extinguished, but immediately re-appears on discontinuing the blast.

The light of bodies is, in general, uncoloured; there are, however, some exceptions. Blue fluor, of that kind which gives out a fetid smell when rubbed, first emits a bright green light, resembling that of the glow-worm so exactly, that when placed by the insect just as it has attained its greatest brightness, there is no sensible difference in the two lights, either of colour or intensity. This bright green quickly changes into a beautiful lilac, which gradually fades away. Fetid marbles, and some kinds of chalk, give a bright reddish or orange light; pure calcareous earth, a bluish white light; Cornish moorstone emits a fine blue light; powder of ruby gives a beautiful red light, of short continuance.

The most phosphorescent marble is soft and friable, of a coarse crystallized grain, and a fetid odour when rubbed; black and grey marbles are generally more luminous than the white. Most of the common white marbles are hard, and of a fine grain, and they are not very luminous, nor is their light of an orange colour. Different chalks vary as much as different marbles, in the intensity and colour of their light, when no difference of external structure is perceptible. The most phosphorescent chalk loses the brilliancy and redness of its light by being dissolved in an acid, and precipitated by caustic fixed vegetable alkali—by being combined with vitriolic or fluor acid —by calcination by heat, or being combined with the aerial acid in the pellicle formed on the surface of lime-water. Marble would probably be affected in a similar manner. The most phosphorescent blue fluor gives the same light after being united to the vitriolic acid, though gypsum is far less luminous than fluor, and its light is colourless. Argil precipitated from alum by an alkali, and magnesia, when combined with fluor acid, give out the same light as before.

Bodies emit their light when immersed in boiling acid of vitriol, or in boiling oils; small lumps of fluor or of marble make a singular appearance in the acid, as they are moved up and down by its action, and rendered brightly luminous by the heat: they seem equally luminous in pure, fixed, inflammable, or atmospheric air.

Feldspat, the fetid fluor, and probably all phosphorescent bodies, dropt, in moderately fine powder, into a flask containing a small quantity of boiling oil at the bottom, emit a copious flash of light as soon as the powder touches the surface of the oil; when the particles of the body have lain at the bottom of the heated fluid for about a minute, they become but faintly luminous; if the flask be then agitated so as to raise some of these particles out of the oil, and lodge them on its sides, they suddenly rekindle into the same brightness as at first, and preserve this re-assumed lustre for some time; and even after being again washed down into the oil, they may be readily distinguished from the particles which have remained at the bottom. This experiment is extremely beautiful, and is not at all obstructed by the faint light of the oil; it succeeds best with the stinking blue fluor of Derbyshire.

Powdered marble, and probably every other body, when spread upon the heater, in the receiver of an air-pump, is equally luminous during the exhaustion and re-admission of the air.

Bodies are by far most luminous the first time they are heated, but cannot, perhaps, be entirely deprived of this property by any number of heatings, nor by any degree of heat. Chalk, fluor, and feldspat, give out a very faint light on the heater, after having been exposed to a smart red heat in an open crucible, in small quantities, and kept frequently stirred for several hours; the feldspat was equally luminous when laid hot upon the heater, or first cooled, and then laid on. Chalk and fluor were not tried in this particular. A bit of glass, melted in a heat of 120° of my father's thermometer, and as soon as it is cold reduced to powder, gives out light on being thrown upon the heater below redness.

Quartz, from the same original piece, is equally luminous when the *powder* is directly thrown upon the heater—when it is previously made red hot, and then cooled and thrown on —or when a *fragment of some size* has been made red hot, then pounded and thrown on.

For the most part, the softest bodies require the least heat to become luminous; marble, chalk, fluor, &c. give a faint light when sprinkled on melted tin just becoming solid. As the temperature of the heater is raised, they continue to give out more and more light.

Vitriols of iron, copper, and zink, previously exsiccated, when thrown on earthen ware or metal made nearly red hot, give minute flashes of light of momentary duration, such as appear from some of the metallic precipitates, particularly zink, on a similar treatment; with this difference, however, that the light of most of the precipitates is of a reddish hue.

The light of the metals is white, and exactly similar to that of some earths.

White paper, when dipped in a solution of sal ammoniac, and slowly dried, becomes black upon the heater, and then gives out much less light than common paper.

If a lump, of the size of a small bean, of fluor, marble, feldspat, or any of the most phosphorescent bodies, be laid upon the heater, the light proceeds gradually upwards from the part in contact with the heater, till the whole mass is thoroughly illuminated; if the same piece be heated a second time, it is much less luminous; nor, if it be broken, are the fragments at all more luminous, either then, or after having been exposed for a month to the light and sunshine.

A little boiling oil at the bottom of a glass flask, when agitated in the dark, illuminates the whole of the flask. The light of boiling oils proceeds, probably, from some kind of inflammation, as it is scarcely discernible unless the vessel be agitated; and, if a little oil be thinly spread on the heater, a subtle lambent flame, of a bluish hue, instantly arises. The same thing takes place if horn, bone, hair, saliva, or any animal matter be laid upon the heater.

II.

The experiments on the light produced from different bodies by attrition, were chiefly made by rubbing in the dark two pieces of the same kind against each other: all that I tried, with a very few exceptions, were luminous by this treatment. The following is a list of them, arranged in the order of the apparent intensity of their light, and as the lights are either white, or some shade of red, I have affixed figures to denote these differences; (0) denoting a pure white light; (1), the faintest tinge of red, or flame colour; (2), a deeper shade of red; (3) and (4), still deeper shades.

1. Colourless, transparent, oriental rock crystal; and siliceous crystals (0).
2. Diamond (0).
3. White quartz; white semitransparent agate (1).
4. White agate, more opake (2).
   Semitransparent feldspat, from Scotland (2).
   Brown opake feldspat, from Saxony (4).
   Chert of a dusky white, from North Wales (3).
5. Oriental ruby (4).
6. Topaz; oriental sapphire (0).
7. Agate, deep coloured, brown and opake (4).

8. Clear, blackish gun-flint (2).
9. Tawney semitransparent flint (3).
10. Unglazed white biscuit earthen ware (4).
11. Fine white porcelain (2).
12. Clear, blackish gun-flint, made opake by heat (3).
13. Flint glass (0).
14. Plate glass ; green bottle glass (0).
15. Fine hard loaf sugar (0).
16. Moorstone, from Cornwall (1).
    Corune, semitransparent, from the East Indies (1).
17. Iceland spar (0).
18. White enamel (2) ; tobacco pipe (3).
    White Mica (0).
19. Unglazed biscuit earthen ware, blackened by exposing it, buried in charcoal in a close crucible, to a white heat (4.)
20. * Black vitreous mass, made by melting together 5 of fluor, 1 of lime, and some charcoal powder (4).
21. Fluor ; aerated and vitriolated barytes ; white and black Derbyshire marble ; calcareous spar ; crystals of borax ; deep blue glass; mother of pearl.

Rock crystal, quartz, flint glass, and many other hard bodies, during attrition, emit now and then reddish sparks of a vivid light, which retain their brightness in a passage of one, two, and even three inches, through the air.

A piece of opake agate, applied to the circumference of a wheel of fine grit, revolving at a moderate rate, becomes

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* Some of this mixture taken out of the crucible before it was perfectly fused, gave out, when rubbed, a strong smell like phosphorus of urine; and on throwing some of it pulverized on a plate of iron, heated just below redness, it was very luminous, and presented every appearance of burning phosphorus.

brightly red, even in day-light, at the touching part; if the wheel revolve at a quicker rate, the touching part emits a pure white light. In both cases, glowing sparks are continually emitted, some of which are not extinguished before they have passed twelve or fourteen inches through the air; they explode gunpowder and inflammable air, and burn the skin; their brightness is not sensibly increased by passing into pure air. The corner of an angular piece of window glass being applied to the wheel in motion, a full eighth of an inch of the glass above the point of contact becomes, apparently, red hot, and retains the redness for a second or two of time after its removal from the wheel; during the attrition, large red sparks are continually emitted, and a mixture of softened glass, and the sand of the stone wheel, is collected about the touching point. Quartz, transparent agate, rock crystal, and window glass, give nearly the same flashing light, when rubbed against the stone wheel, or in the ordinary manner, excepting the tinge of red in the former, which it receives from the light of the grit: the transparent agate becomes red hot for a little way about the part in contact with the wheel, and is thus deprived of its transparency, as it would be if made red hot in a common fire; porcelain is heated to redness by the same treatment. The red sparks which are emitted by all these bodies during their attrition, are heated particles about the magnitude of grains of fine sand, broken off by the friction.

Bodies give out their light the instant they are rubbed upon each other, and cease to be luminous when the attrition is discontinued. Colourless, transparent, and semitransparent bodies emit a flashing light, their whole masses being, for a moment, illuminated; opake bodies give little more than a defined speck of red light, and are not luminous below the part struck. The greatest apparent quantity of light is produced by hard, uncoloured, transparent, and semitransparent bodies, whose surfaces soon acquire an asperity by rubbing together, as quartz, agate, &c. From an examination of the table, it appears that white lights are emitted from colourless transparent bodies; faint red, or flame-coloured, from white semitransparent bodies; deeper red from more opake and coloured bodies, and the deepest red from opake and from deep-coloured bodies. Extremely faint lights, such as those given by fluor, marble, &c. are of a bluish white; quartz, very lightly rubbed, gives a very faint light of a bluish hue; when rubbed a little harder, it emits a flame-coloured light; when rubbed with violence, its light approaches to whiteness. Opake red feldspat gives a deep red light by attrition; exposed to a strong heat in the furnace, it becomes white, and somewhat transparent, and when cool, gives out, on attrition, as white a light as quartz; clear, blackish flint, made opake by heat, gives a redder light than before; deep-coloured glass gives out a red defined light without any flash, whilst clear uncoloured glasses emit a white flashing light of some brightness.

Bodies are not luminous by simple pressure; but when they are at all broken by the pressure, the fragments rubbing on each other produce some light. Mr. Boyle, indeed, found a particular diamond to emit light when pressed by a steel bodkin; but the diamond is phosphorescent in so many ways, and is so curious and singular a body, both in properties and constitution, that it can scarcely be expected to exhibit the same appearances as the common class of earthy bodies.

Alum, indurated by having been kept long in a state of fusion, and being then much harder than loaf sugar or borax, both which are luminous from moderate attrition, gives no light, though rubbed with much violence*.

If two pieces of glass or quartz be strongly rubbed against each other, and then applied to the fine down of a feather, the down is not sensibly affected; if the same glass be rubbed on woollen cloth, and placed near the feather, the down is immediately attracted.

Rock crystal, quartz, feldspat, white unglazed earthen ware, Derbyshire black marble, and probably all phosphorescent bodies, insoluble in water, give out their light on rubbing them under water, as copiously as in air. Hard white sugar, from the outside of the loaf, gives out its light when rubbed in oil. Bodies seem equally luminous in atmospheric, pure, fixed, and inflammable, air.

All hard earthy bodies emit a peculiar smell on attrition. The most remarkable for this property are chert, quartz, feldspat, biscuit earthen ware, and rock crystal: this smell does not differ much in kind, though it does considerably in intensity. Many of the softer bodies yield the same smell, but in a less degree, and, probably, none are entirely without it. It appears to be strongest where the friction is greatest: it has no dependence on the light produced by attrition, as it is often very strong when no light is emitted.

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* The Count DE RAZOUMOWSKI has investigated the luminous property of bodies in a way which appears to me very unfavourable for the discovery of their true lights. He rubbed, not one piece against another of the same body, but, all of them against quartz or glass: he finds several metals luminous from this treatment, and attempts to draw some curious conclusions from the colour of their lights. I tried these metals in his own way, and found that no light was emitted except when the violence of the blow shattered the quartz or glass; a piece of the indurated alum will excite light from rock crystal, by breaking its surface, but this is the light of the fragments of the crystal rubbing on each other, and not of the alum.

Rock crystal, quartz, feldspat, white biscuit earthen ware, and probably all such hard bodies, produce this smell under water.

Quartz stones, violently rubbed upon one another for a few minutes in a cup of water, communicate this smell, and a peculiar taste, to the water. The taste is probably derived from an impalpable powder, which floats in the water for many days.

Derbyshire black marble, and the stinking blue fluor, give out, on attrition, a strong smell peculiar to themselves, both in air and water; they lose this property by being once made red hot.

Quartz produces the smell equally strong in fixed, pure, and common, air.

Having now stated all the facts relative to phosphorescent bodies which I have as yet been able to discover, I shall beg leave to offer a few reflections, tending to shew, that heat is the probable cause of the light produced from bodies by attrition.

The powders of all earthy bodies emit light when heated a little under redness. Now, when two bodies are rubbed upon each other, it is probable that heat is *always* generated on their surfaces: may not then the light which they yield upon attrition be attributed to a sudden heating of particles in their surfaces? for these particles will be affected in the same way as if they had been equally heated by any other means; they will therefore give the same light as if they had been laid upon the heater of an equal temperature.

The shining sparks which hard bodies send out during attrition, prove, that particles in their surfaces are heated at least to more than 600° of Fahrenheit, for the powders of hard bodies are not luminous on a heater much below redness. The heat generated by the attrition of soft friable bodies is probably but little, as the minute particles in their surfaces are not much crushed by each other, but merely disjoined from the masses; nor can this little be easily appreciated, for as the surfaces continually crumble away, the heated parts are carried off before they can warm the masses, and are themselves very quickly cooled by the surrounding air. But as many soft bodies emit a faint light on the heater at the low temperature of about 400° of Fahrenheit, and as it is not a stronger light which they yield on attrition, little heat is required to render the particles in their surfaces luminous. It must be observed too, that, though the absolute quantity of heat generated by one rub be but inconsiderable, the effects of it may be very striking; for just on the instant of attrition, its action is confined to the minute colliding points of the surfaces, and will consequently operate upon them as much as a greater quantity would on larger points.

The light emitted by bodies in attrition is of momentary duration, whereas a powder on the heater continues to emit light for some minutes: this difference is easily explained; in the latter case, the particles are constantly heated; in the former, they are instantly cooled by the subjacent mass to a temperature in which bodies are not luminous, *i. e.* a little under 400° of Fahrenheit.

When the uneven surfaces of bodies are rubbed upon each other, a flashing light is produced at frequent intervals, by the collision of the more prominent parts; this, in colourless, transparent, and semitransparent bodies, is copiously reflected from the whole of the masses, and forms an appearance very different from what is ever exhibited by bodies on the heater: it may be well illustrated in the upper part of a candle, by repeatedly nearly closing, and quickly opening, the snuffers, about the burning wick; or by sprinkling some powder of fluor or marble on a mass of glass heated just under redness.

Powder of crystal, quartz, agate, &c. is but faintly luminous on the heater under redness; accordingly, if the stones themselves be gently rubbed, a faint light is emitted, resembling that which their powders give out on the heater. Marble and fluor give about the same light on the heater of the temperature of 400°, as they do when rubbed; and probably attrition heats some particles in their surfaces to that degree.

It may, at first, seem an objection to the opinion of heat being the cause of the light produced from bodies by attrition, that they yield their light when rubbed under water; but the water acts differently to the air, only, as being a stronger conductor of heat, and can nowise impede the actual generation of the heat; now, as bodies emit their light on the very instant of their being heated, the water cannot cool the hot particles before they have given their light.

It is easy to see why bodies emit light *instantly* when rubbed; for they often send out sparks as soon as the attrition commences, which proves that particles in their surfaces are *instantly* heated to redness by attrition.

Since hard bodies may be heated to redness by attrition, we have an excellent method of discovering the lights they give out at that temperature, which could not be effected by sprinkling their powders on a red hot heater, as the light of the powder would be mixed with that of the heater. In some cases of attrition, bodies are raised to a temperature beyond visible red heat. The corner of an angular piece of window glass being applied to the circumference of a revolving wheel of fine grit, part of its mass is worn away; but a larger portion, lying just above the abraded part, is heated to redness. Now, as all the heat which is there collected, and a great deal more, which is carried away in the abraded part, and conducted off by the air, and by the glass lying up to the red hot portion, has once occupied a smaller space in the part worn away; it follows, that the abraded portion, or aggregate of heated surfaces, has been heated to a degree exceeding redness, by all the heat remaining in the red hot part, and by the quantity of heat conducted off by the air and the adjacent glass; and, consequently, that each surface has been heated by the attrition to a degree *as* much exceeding redness.

I am aware that this reasoning is founded, in part, on the supposition that the heat is generated on the surface or outermost coat of the body: some of it may undoubtedly proceed from an agitation of parts under the surface; but the emission of red hot sparks at the *instant* of attrition, proves that a great heat is generated on the surface; and, as the friction, or apparent heating cause, is so much greater there, that the parts are forcibly broken and disjoined, whilst just beneath there is no perceptible alteration in the body, we may venture to conclude that the heat generated beneath the surface is but inconsiderable.

After all, it remains entirely problematical, in what manner heat operates to produce light from bodies: the air does not seem to have any concern in its production, as bodies are equally luminous in almost all kinds of air, and when immersed in liquids. The phosphorism of sugar is probably of a different kind from that of the earthy class; for, though so soft and friable a substance, it produces its light very copiously upon gentle attrition.

In speaking of the attrition of bodies upon the stone wheel, I have said that they became *red hot* about the touching part; I should not have made use of this expression if the luminous sparks, which issued from them, had not kindled gunpowder and inflammable air, and thus proved that the part from which they came was raised to a temperature, at least equal to what is usually termed a *red heat*; for till the temperature of the part touching the wheel had been thus ascertained, I attributed the whole of the light emitted to the common phosphorism of bodies. If the velocity of the wheel be much increased, the touching part of the body applied, emits a bright white light, much more vivid than any which powders ever give out on the heater, and, probably, the temperature of the luminous part is equal to what is usually called a *white heat*.

Having thus made incombustible bodies red hot without the aid of fire, I once conceived that all the light which they emit when heated to redness in the fire, proceeded entirely from their great phosphorism; for I could not suppose that they absorbed light from the burning fuel and emitted it again, at the same time, and during a continuance of the same circumstances. It appeared, however, equally inexplicable, why a stone put into the fire, should continue to shine from its own light, with undiminished lustre, as long as the fire is kept up; for it has been shewn, that if a phosphorescent body remain long upon the heater, of any temperature between 400° of Fahrenheit and a red heat, its light diminishes more and more, till at last it is scarcely perceptible; and then an increase of heat is necessary to render it more luminous.

APPENDIX.

After a considerable part of the above paper was printed, I repeated the experiment with boiling oil, related in p. 35, with every possible precaution. I poured the powder into the flask through a funnel which reached to the bottom, so that none of it might be lodged on the sides; for, not having attended to this circumstance before, I was apprehensive that the experiment was delusive, and that the agitation of the hot oil might have washed down some fresh particles which had not been before heated;—then slowly pouring in a little oil, I boiled it for a few minutes, and removed it into a dark place. When the powder had become but faintly luminous, upon agitation, the experiment succeeded exactly. I then boiled and agitated the oil for six or seven times successively, with the same result, except that the light of the powder grew something fainter each time. If any one has the curiosity to repeat this singular experiment, he may attend to the following directions.—Upon each removal from the fire, stop the neck of the flask with a cork, having a small hole pierced through it—wrap the neck round with tow—agitate the liquor by quickly raising and lowering the flask.

The sparks which leave the surfaces of hard bodies during attrition, may be exactly imitated by burning a bit of dusty paper;—the particles of dust are carried by the current of air through the blaze, and are made red hot in their passage.

IV. *Experiments upon Heat. By Major-General Sir* Benjamin Thompson, *Knt. F.R.S. In a Letter to Sir* Joseph Banks, *Bart. P.R.S.*

Read January 19, 1792.

DEAR SIR, *Munich, June,* 1787.

SINCE my last communication upon the subject of HEAT, which the Royal Society have done me the honour to publish in their Transactions, I have made some further progress in the investigation of that most interesting subject, of which I propose to give you an account in this letter.

The confining and directing of heat are objects of such vast importance in the œconomy of human life, that I have been induced to confine my researches chiefly to those points, conceiving that very great advantages to mankind could not fail to be derived from the discovery of any new facts relative to these operations.

If the laws of the communication of heat from one body to another were known, measures might be taken with certainty, in all cases, for confining it, and directing its operations, and this would not only be productive of great œconomy in the articles of fuel and clothing, but would likewise greatly increase the comforts and conveniencies of life; objects of which the philosopher should never lose sight.

The route which I have followed in this inquiry is that which I thought bid fairest to lead to useful discoveries. Without embarrassing myself with any particular theory, I have formed to myself a plan of experimental investigation, which I conceived would conduct me to the knowledge of certain facts, of which we are now ignorant, or very imperfectly informed, and with which it is of consequence that we should be made acquainted.

The first great object which I had in view in this inquiry was to ascertain, if possible, the cause of the warmth of certain bodies; or the circumstances upon which their power of confining heat depends. This, in other words, is no other than to determine the cause of the conducting and non-conducting power of bodies.

To this end I began by determining by actual experiment the relative conducting powers of various bodies of very different natures, both fluids and solids, of some of which experiments I have already given an account in the Paper above mentioned, which is published in the Transactions of the Royal Society for the year 1786; I shall now, taking up the matter where I left it, give you the continuation of the history of my researches.

Having discovered that the Torricellian vacuum is a much worse conductor of heat than common air, and having ascertained the relative conducting powers of air, of water, and of mercury, under different circumstances, I proceeded to examine the conducting powers of various solid bodies, and particularly of such substances as are commonly made use of for clothing.

The method of making these experiments was as follows: a mercurial thermometer *, whose bulb was about $\frac{55}{100}$ of an inch

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* See Phil. Trans. Vol. LXXVI. Tab. VI. Fig. 4.

in diameter, and its tube, about 10 inches in length, was suspended in the axis of a cylindrical glass tube, about $\frac{3}{4}$ of an inch in diameter, ending with a globe $1\frac{6}{10}$ inch in diameter, in such a manner that the centre of the bulb of the thermometer occupied the centre of the globe; and the space between the internal surface of the globe and the surface of the bulb of the thermometer being filled with the substance whose conducting power was to be determined, the instrument was heated in boiling water, and afterwards being plunged into a freezing mixture of pounded ice and water, the times of cooling were observed, and noted down.

The tube of the thermometer was divided at every tenth degree from 0°, or the point of freezing, to 80°, that of boiling water, and these divisions being marked upon the tube with the point of a diamond, and the cylindrical tube being left empty, the height of the mercury in the tube of the thermometer was seen through it.

The thermometer was confined in its place by means of a stopple of cork, about $1\frac{1}{2}$ inch long, fitted to the mouth of the cylindrical tube, through the centre of which the end of the tube of the thermometer passed, and in which it was cemented.

The operation of introducing into the globe the substances whose conducting powers are to be determined, is performed in the following manner; the thermometer being taken out of the cylindrical tube, about two-thirds of the substance which is to be the subject of the experiment are introduced into the globe; after which, the bulb of the thermometer is introduced a few inches into the cylinder; and, after it, the remainder of the substance being placed round about the tube of the thermometer; and, lastly, the thermometer being introduced farther into the tube, and being brought into its proper place, that part of the substance which, being introduced last, remains in the cylindrical tube above the bulb of the thermometer, is pushed down into the globe, and placed equally round the bulb of the thermometer by means of a brass wire which is passed through holes made for that purpose in the stopple closing the end of the cylindrical tube.

As this instrument is calculated merely for measuring the passage of heat in the substance whose conducting power is examined, I shall give it the name of *passage-thermometer*, and I shall apply the same appellation to all other instruments constructed upon the same principles, and for the same use, which I may in future have occasion to mention; and as this instrument has been so particularly described, both here, and in my former Paper upon the subject of heat, in speaking of any others of the same kind in future it will not be necessary to enter into such minute details. I shall, therefore, only mention their *sizes*, or the diameters of their bulbs, the diameters of their globes, the diameters of their cylinders, and the lengths and divisions of their tubes, taking it for granted that this will be quite sufficient to give a clear idea of the instrument.

In most of my former experiments, in order to ascertain the conducting power of any body, the body being introduced into the globe of the passage-thermometer, the instrument was cooled to the temperature of freezing water, after which, being taken out of the ice water, it was plunged suddenly into boiling water, and the times of heating from ten to ten degrees were observed and noted; and I said that these times were as the conducting power of the body inversely; but in the experiments of which I am now about to give an account, I have in general reversed the operation; that is to say, instead of observing the times of heating, I have first heated the body in boiling water, and then plunging it into a mixture of pounded ice and ice-cold water, I have noted the times taken up in cooling.

I have preferred this last method to the former, not only on account of the greater ease and convenience with which a thermometer, plunged into a mixture of water, may be observed, than when placed in a vessel of boiling water, and surrounded by hot steam, but also on account of the greater accuracy of the experiment, the heat of boiling water varying with the variations of the pressure of the atmosphere: consequently the experiments made upon different days will have different results, and of course, strictly speaking, cannot be compared together; but the temperature of pounded ice and water is ever the same, and of course the results of the experiments are uniform.

In heating the thermometer, I did not in general bring it to the temperature of the boiling water, as this temperature, as I have just observed, is variable; but when the mercury had attained the 75° of its scale, I immediately took it out of the boiling water, and plunged it into the ice and water; or, which I take to be still more accurate, suffering the mercury to rise a degree or two above 75°, and then taking it out of the boiling water, I held it over the vessel containing the pounded ice and water, ready to plunge it into that mixture the moment the mercury, descending, passes the 75°.

Having a watch at my ear which beat half seconds (which I counted), I noted the time of the passage of the mercury over the divisions of the thermometer, marking 70° and every tenth degree from it, descending, to 10° of the scale. I continued the cooling to 0°, or the temperature of the ice and water, in very few instances, as this took up much time, and was attended with no particular advantage, the determination of the times taken up in cooling 60 degrees of REAUMUR'S scale, that is to say, from 70° to 10°, being quite sufficient to ascertain the conducting power of any body whatever.

During the time of cooling in ice and water, the thermometer was constantly moved about in this mixture from one place to another; and there was always so much pounded ice mixed with the water, that the ice appeared above the surface of the water; the vessel, which was a large earthen jar, being first quite filled with pounded ice, and the water being afterwards poured upon it; and fresh quantities of pounded ice being added as the occasion required.

Having described the apparatus made use of in these experiments, and the manner of performing the different operations, I shall now proceed to give an account of the experiments themselves.

My first attempt was to discover the relative conducting powers of such substances as are commonly made use of for clothing; accordingly, having procured a quantity of *raw silk*, as spun by the worm, *sheep's wool, cotton wool, linen* in the form of the finest lint, being the scrapings of very fine Irish linen, the finest part of the *fur of the beaver*, separated from the skin, and from the long hair, the finest part of the *fur of a white Russian hare*, and *Eider down*; I introduced successively 16 grains in weight of each of these substances into the globe of the passage-thermometer, and placing it carefully and equally round the bulb of the thermometer, I heated the thermometer in boiling water, as before described, and taking it out of the boiling water, plunged it into pounded ice and water, and observed the times of cooling.

But as the interstices of these bodies thus placed in the globe were filled with air, I first made the experiment with air alone, and took the result of that experiment, as a standard by which to compare all the others; the results of three experiments with air were as follows:

The bulb of the thermometer surrounded by air.

Heat lost.	Exp. No. 1.	Exp. No. 2.	Heat acquired.	Exp. No. 3.
	Time elapsed.	Time elapsed.		Time elapsed.
70°	—	—	10°	—
60°	38″	38″	20°	39″
50°	46	46	30°	43
40°	59	59	40°	53
30°	80	79	50°	67
20°	122	122	60°	96
10°	231	230	70°	175
Total times.	576	574	—	473

The following table shows the results of the experiments, with the various substances therein mentioned:

Heat lost.	Air.	Raw silk, 16 grs.	Sheep's wool, 16 grs.	Cotton wool, 16 grs.	Fine lint, 16 grs.	Beavers fur, 16 grs.	Hares fur, 16 grs.	Eider down, 16 grs.
	Exp. 1.	Exp. 4.	Exp. 5.	Exp. 6.	Exp. 7.	Exp. 8.	Exp. 9.	Exp. 10.
70°	—	—	—	—	—	—	—	—
60°	38″	94″	79″	83″	80″	99″	97″	98″
50°	46	110	95	95	93	116	117	116
40°	59	133	118	117	115	153	144	146
30°	80	185	162	152	150	185	193	192
20°	122	273	238	221	218	265	270	268
10°	231	489	426	378	376	478	494	485
Total times.	576	1284	1118	1046	1032	1296	1315	1305

Now the *warmth* of a body, or its power to confine heat, being as its power of resisting the passage of heat through it (which I shall call its *non-conducting power*), and the time taken up by any body in cooling, which is surrounded by any medium through which the heat is obliged to pass, being, *cæteris paribus*, as the resistance which the medium opposes to the passage of the heat, it appears that the *warmth* of the bodies mentioned in the foregoing table are as the times of cooling; the *conducting powers* being inversely as those times, as I have formerly shown.

From the results of the foregoing experiments it appears, that of the seven different substances made use of, hares fur and Eider down were the warmest; after these came beavers fur; raw silk; sheep's wool; cotton wool; and lastly, lint, or the scrapings of fine linen; but I acknowledge that the differences in the warmth of these substances were much less than I expected to have found them.

Suspecting that this might arise from the volumes or solid contents of the substances being different (though their weights were the same), arising from the difference of their specific gravities; and as it was not easy to determine the specific gravities of these substances with accuracy, in order to see how far any known difference in the volume or quantity of the same substance, confined always in the same space, would add to, or diminish, the time of cooling, or the apparent warmth of the covering, I made the three following experiments.

In the first, the bulb of the thermometer was surrounded by 16 grains of Eider down; in the second by 32 grains; and in the third by 64 grains; and in all these experiments the substance was made to occupy exactly the same space, viz. the whole internal capacity of the glass globe, in the centre of which the bulb of the thermometer was placed; consequently the thickness of the covering of the thermometer remained the same, while its density was varied in proportion to the numbers 1, 2, and 4.

The results of these experiments were as follow :

The bulb of the thermometer being surrounded by Eider down.			
Heat lost.	16 grains.	32 grains.	64 grains.
	(Exp. No. 11.)	(Exp. No. 12.)	(Exp. No. 13.)
70°	—	—	—
60°	97″	111″	112″
50°	117	128	130
40°	145	157	165
30°	192	207	224
20°	267	304	326
10°	486	565	658
Total times	1304	1472	1615

Without stopping at present to draw any particular conclusions from the results of these experiments, I shall proceed to give an account of some others, which will afford us a little further insight into the nature of some of the circumstances upon which the warmth of covering depends.

Finding, by the last experiments, that the density of the covering added so considerably to the warmth of it, its thickness remaining the same, I was now desirous of discovering how far the internal structure of it contributed to render it more or less pervious to heat, its thickness and quantity of matter remaining the same. By internal structure, I mean the disposition of the parts of the substance which forms the covering; thus they may be extremely divided, or very fine, as raw silk as spun by the worms, and they may be equally distributed through the whole space they occupy; or they may be coarser, or in larger masses, with larger interstices, as the ravelings of cloth, or cuttings of threads.

If heat passed *through* the substances made use of for covering, and if the warmth of the covering depended solely upon the difficulty which the heat meets with in its passage through the substances, *or solid parts,* of which they are composed; in that case, the warmth of covering would be always, *cæteris paribus*, as the quantity of materials of which it is composed; but that this is not the case, the following, as well as the foregoing experiments clearly evince.

Having, in the experiment N° 4, ascertained the warmth of 16 grains of raw silk, I now repeated the experiment with the same quantity, or weight, of the ravelings of white taffety, and afterwards with a like quantity of common sewing silk, cut into lengths of about two inches.

The following table shows the results of these three experiments:

Heat lost.	Raw silk, 16 grs.	Ravelings of taffety, 16 grs.	Sewing silk cut into lengths, 16 grs.
	Exp. 4.	Exp. 14.	Exp. 15.
70°	—	—	—
60°	94″	90″	67″
50°	110	106	79
40°	133	128	99
30°	185	172	135
20°	273	246	195
10°	489	427	342
Total times.	1284	1169	917

Here, notwithstanding that the quantities of the silk were the same in the three experiments, and though in each of them it was made to occupy the same space, yet the warmth of the coverings which were formed were very different, owing to the different disposition of the material.

The raw silk was very fine, and was very equally distributed through the space it occupied, and it formed a warm covering.

The ravelings of taffety were also fine, but not so fine as the raw silk, and of course the interstices between its threads were greater, and it was less warm ; but the cuttings of sewing silk were very coarse, and consequently it was very unequally distributed in the space in which it was confined ; and it made a very bad covering for confining heat.

It is clear from the results of the five last experiments, that the air which occupies the interstices of bodies, made use of for covering, acts a very important part in the operation of confining heat ; yet I shall postpone the examination of that circumstance till I shall have given an account of several other experiments, which, I think, will throw still more light upon that subject.

But, before I go any farther, I will give an account of three experiments which I made, or rather the same experiment which I repeated three times the same day, in order to see how far experiments made according to the method here described, may be depended on, as being regular in their results.

The glass globe of the passage-thermometer being filled with 16 grains of cotton-wool, the instrument was heated and cooled three times successively, when the times of cooling were observed as follows :

Heat lost.	Exp. 16.	Exp. 17.	Exp. 18.
70°	—	—	—
60°	82″	84″	83″
50°	96	95	95
40°	118	117	116
30°	152	153	151
20°	221	221	220
10°	380	377	377
Total times.	1049	1047	1042

The difference of the times of cooling in these three experiments were extremely small; but, regular as these experiments appear to have been in their results, they were not more so than the other experiments made in the same way, many of which were repeated two or three times, though, for the sake of brevity, I have put them down as single experiments.

But to proceed in the account of my investigations relative to the causes of the warmth of warm clothing. Having found that the fineness and equal distribution of a body or substance made use of to form a covering to confine heat, contributes so much to the warmth of the covering, I was desirous, in the next place to see the effect of condensing the covering, its quantity of matter remaining the same, but its thickness being diminished in proportion to the increase of its density.

The experiment I made for this purpose was as follows:—I took 16 grains of common sewing silk, neither very fine nor very coarse, and winding it about the bulb of the thermometer in such a manner that it entirely covered it, and was as nearly as possible of the same thickness in every part, I replaced the thermometer in its cylinder and globe, and heating it in boiling water, cooled it in ice and water, as in the foregoing experiments. The results of the experiment were as may be seen in the following table; and in order that it may be compared with those made with the same quantity of silk differently disposed of, I have placed those experiments by the side of it:

Heat lost.	Raw silk, 16 grs.	Fine ravelings of taffety, 16 grs.	Sewing silk cut into lengths, 16 grs.	Sewing silk, 16 grs. wound round the bulb of the thermometer.
	Exp. No. 4.	Exp. No. 14.	Exp. No. 15	Exp. No. 19.
70°	—	—	—	—
60°	94″	90″	67″	46″
50°	110	106	79	62
40°	133	128	99	85
30°	185	172	135	121
20°	273	246	195	191
10°	489	427	342	399
Total times.	1284	1169	917	904

It is not a little remarkable, that, though the covering formed of sewing silk wound round the bulb of the thermometer in the 19th experiment, appeared to have so little power of confining the heat when the instrument was very hot, or when it was first plunged into the ice and water, yet afterwards, when the heat of the thermometer approached much nearer to that of the surrounding medium, its power of confining the heat which remained in the bulb of the thermometer appeared to be even greater than that of the silk in the experiment N° 15, the time of cooling from 20° to 10° being in the one 399″, and in the other 342″. The same appearance was observed in the following experiments, in which the bulb of the thermometer was surrounded by threads of *wool*, of *cotton*, and of *linen*, or *flax*, wound round it, in the like manner as the sewing silk was wound round it in the last experiment.

The following table shows the results of these experiments, with the threads of various kinds; and that they may the more easily be compared with those made with the same quantity of the same substances in a different form, I have placed the accounts of these experiments by the side of each other. I have also added the account of an experiment, in which 16 grains of fine linen cloth were wrapped round the bulb of the thermometer, going round it nine times, and being bound together at the top and bottom of it, so as completely to cover it.

Heat lost.	*Sheep's wool*, 16 grains, surrounding the bulb of the thermometer.	*Woollen thread*, 16 grains, wound round the bulb of the thermometer.	*Cotton wool*, 16 grains, surrounding the bulb of the thermometer.	*Cotton thread*, 16 grains, wound round the bulb of the thermometer.	*Lint*, 16 grains, surrounding the bulb of the thermometer.	*Linen thread*, 16 grains, wound round the bulb of the thermometer.	*Linen cloth*, 16 grains, wrapped round the bulb of the thermometer.
	Exp. 5.	Exp. 20.	Exp. 6.	Exp. 21.	Exp. 7.	Exp. 22.	Exp. 23.
70°	—	—	—	—	—	—	—
60°	79″	46″	83″	45″	80″	46″	42″
50°	95	63	95	60	93	62	56
40°	118	89	117	83	115	83	74
30°	162	126	152	115	150	117	108
20°	238	200	221	179	218	180	168
10°	426	410	378	370	376	385	338
Tot. times.	1118	934	1046	852	1032	873	783

That thread wound light round the bulb of the thermometer should form a covering less warm than the same quantity of wool, or other raw materials of which the thread is made, surrounding the bulb of the thermometer in a more loose manner, and consequently occupying a greater space, is no more than what I expected, from the idea I had formed of the causes of the warmth of covering; but I confess I was much surprised to find that there is so great a difference in the relative warmth of these two coverings, when they are employed to confine great degrees of heat, and when the heat they confine is much less in proportion to the temperature of the surrounding medium. This difference was very remarkable; in the experiments with sheep's wool, and with woollen thread, the warmth of the covering formed of 16 grains of the former, was to that formed of 16 grains of the latter, when the bulb of the thermometer was heated to 70° and cooled to 60°, as 79 to 46 (the surrounding medium being at 0°); but afterwards, when the thermometer had only fallen from 20° to 10° of heat, the warmth of the wool was to that of the woollen thread only as 426 to 410; and in the experiments with lint, and with linen thread, when the heat was much abated, the covering of the thread appeared to be even warmer than that of the lint, though in the beginning of the experiments, when the heat was much greater, the lint was warmer than the thread, in the proportion of 80 to 46.

From hence it should seem that a covering may, under certain circumstances, be very good for confining small degrees of warmth, which would be but very indifferent when made use of for confining a more intense heat, and *vice versa*. This, I believe, is a new fact; and, I think the knowledge of it may lead to further discoveries relative to the causes of the warmth of coverings, or the manner in which heat makes its passage through them. But I forbear to enlarge upon this subject, till I shall have given an account of several other experiments, which I think throw more light upon it, and which will consequently render the investigation easier and more satisfactory.

With a view to determine how far the power which certain bodies appear to possess of confining heat, when made use of as covering, depends upon the natures of those bodies, considered as chymical substances, or upon the chymical principles of which they are composed, I made the following experiments.

As charcoal is supposed to be composed almost entirely of phlogiston, I thought that, if that principle was the cause either of the conducting power, or the non-conducting power of the bodies which contain it, I should discover it by making the experiment with charcoal, as I had done with various other bodies. Accordingly, having filled the globe of the passage-thermometer with 176 grains of that substance in very fine powder (it having been pounded in a mortar, and sifted through a fine sieve), the bulb of the thermometer being surrounded by this powder, the instrument was heated in boiling water, and being afterwards plunged into a mixture of pounded ice and water, the times of cooling were observed as mentioned in the following table. I afterwards repeated the experiment with lampblack, and with very pure, and very dry wood ashes; the results of which experiments were as under mentioned:

The bulb of the thermometer surrounded by				
Heat lost.	176 grains of fine powder of charcoal.	176 grains of fine powder of charcoal.	195 grains of lampblack.	307 grains of pure dry wood ashes.
	Exp. No. 24.	Exp. No. 25.	Exp. No. 26.	Exp. No. 27.
70°	—	—	—	—
60°	79″	91″	124″	96″
50°	95	91	118	92
40°	100	109	134	107
30°	139	133	164	136
20°	196	192	237	185
10°	331	321	394	311
Total times.	940	937	1171	927

The experiment, N° 25, was simply a repetition of that numbered 24, and was made immediately after it; but, in moving the thermometer about in the former experiment, the powder of charcoal which filled the globe was shaken a little together, and to this circumstance I attribute the difference in the results of the two experiments.

In the experiments with lampblack and with wood ashes, the times taken up in cooling from 70° to 60° were greater than those employed in cooling from 60° to 50°; this most probably arose from the considerable quantity of heat contained by these substances, which was first to be disposed of, before they could receive and communicate to the surrounding medium that which was contained by the bulb of the thermometer.

The next experiment I made was with *semen lycopodii*, commonly called witch-meal, a substance which possesses very extraordinary properties. It is almost impossible to wet it; a quantity of it strewed upon the surface of a basin of water, not only swims upon the water without being wet, but it prevents other bodies from being wet which are plunged into the water through it; so that a piece of money, or other solid body, may be taken from the bottom of the basin by the naked hand, without wetting the hand; which is one of the tricks commonly shown by the jugglers in the country: this meal covers the hand, and descending along with it to the bottom of the basin, defends it from the water. This substance has the appearance of an exceeding fine, light, and very moveable yellow powder, and it is very inflammable; so much so, that being blown out of a quill into the flame of a candle, it flashes like gunpowder, and it is made use of in this manner in our theatres for imitating lightning.

Conceiving that there must have been a strong attraction between this substance and air, and suspecting, from some circumstances attending some of the foregoing experiments, that the warmth of a covering depends not merely upon the fineness of the substance of which the covering is formed, and the disposition of its parts, but that it arises in some measure from a certain attraction between the substance and the air which fills its interstices, I thought that an experiment with *semen lycopodii* might possibly throw some light upon this matter; and in this opinion I was not altogether mistaken, as will appear by the results of the three following experiments.

The bulb of the thermometer surrounded by 256 grains of *semen lycopodii.*

Heat lost.	Cooled.	Cooled.	Heat acquired.	Heated.
	Exp. No. 28.	Exp. No. 29.		Exp. No. 30.
70°	—	—	0°	—
60°	146″	157″	10°	230″
50°	162	160	20°	68
40°	175	170	30°	63
30°	209	203	40°	76
20°	284	288	50°	121
10°	502	513	60°	316
—	—	—	70°	1585
Total times.	1478	1491	—	2459

In the last experiment (N° 30) the result of which was so very extraordinary, the instrument was cooled to 0° in thawing ice, after which it was plunged suddenly into boiling water, where it remained till the inclosed thermometer had acquired the heat of 70°, which took up no less than 2456 seconds, or above 40 minutes; and it had remained in the boiling water full a minute and an half before the mercury in the thermometer shewed the least sign of rising. Having at length been put into motion, it rose very rapidly 40 or 50 degrees, after which its motion gradually abating became so slow, that it took up 1585 seconds, or something more than 26 minutes, in rising from 60° to 70°, though the temperature of the medium in which it was placed during the whole of this time was very nearly 80°; the mercury in the barometer standing but little short of 27 Paris inches.

All the different substances which I had yet made use of in these experiments for surrounding or covering the bulb of the thermometer, fluids excepted, had, in a greater, or in a less degree, confined the heat, or prevented its passing into or out of the thermometer so rapidly as it would have done, had there been nothing but air in the glass globe, in the centre of which the bulb of the thermometer was suspended. But the great question is, how, or in what manner, they produced this effect?

And first, it was not in consequence of their own non-conducting powers, simply considered; for, if instead of being only bad conductors of heat, we suppose them to have been totally impervious to heat, their volumes or solid contents were so exceedingly small in proportion to the capacity of the globe in which they were placed, that, had they had no effect whatever upon the air filling their interstices, that air would have been sufficient to have conducted all the heat communicated, in less time than was actually taken up in the experiment.

The diameter of the globe being 1,6 inches, its contents amounted to 2,14466 cubic inches; and the contents of the bulb of the thermometer being only 0,08711 of a cubic inch, (its diameter being 0,55 of an inch) the space between the bulb of the thermometer and the internal surface of the globe amounted to 2,1446—60,08711=2,05755 cubic inches; the whole of which space was occupied by the substances by which the bulb of the thermometer was surrounded in the experiments in question.

But though these substances occupied this space, they were far from filling it; by much the greater part of it being filled by the air which occupied the interstices of the substances in question. In the experiment N° 4, this space was occupied by 16 grains of raw silk; and as the specific gravity of raw silk is to that of water as 1734 to 1000, the volume of this silk was equal to the volume of 9,4422 grains of water; and as 1 cubic inch of water weighs 253,185 grains, its volume was equal to $\frac{9,4422}{253,1850}=0,037294$ of a cubic inch; and, as the space it occupied amounted to 2,05755 cubic inches, it appears that the silk filled no more than about $\frac{1}{55}$ part of the space in which it was confined, the rest of that space being filled with air.

In the experiment N° 1, when the space between the bulb of the thermometer and the glass globe, in the centre of which it was confined, was filled with nothing but air, the time taken up by the thermometer in cooling from 70° to 10° was 576 seconds; but in the experiment N° 4, when this same space was filled with 54 parts air, and 1 part raw silk, the time of cooling was 1284 seconds.

Now, supposing that the silk had been totally incapable of conducting any heat at all, if we suppose, at the same time, that it had no power to prevent the air remaining in the globe from conducting it, in that case its presence in the globe could only have prolonged the time of cooling in proportion to the quantity of the air it had displaced to the quantity remaining, that is to say, as 1 is to 54, or a little more than 10 seconds. But the time of cooling was actually prolonged 708 seconds, (for in the experiment N° 1, it was 576 seconds, and in the experiment N° 4, it was 1284 seconds, as has just been observed); and this shows, that the silk not only did not conduct the heat itself, but that it prevented the air by which its interstices were filled from conducting it; or, at least, it greatly weakened its power of conducting it.

The next question which arises is, how air can be prevented from conducting heat? and this necessarily involves another, which is, how does air conduct heat?

If air conducted heat, as it is probable that the metals and water, and all other solid bodies and unelastic fluids conduct it, that is to say, if its particles remaining in their places, the heat passed from one particle to another, through the whole mass, as there is no reason to suppose that the propagation of heat is necessarily in right lines, I cannot conceive how the interposition of so small a quantity of any solid body as $\frac{1}{55}$ part of the volume of the air, could have effected so remarkable a diminution of the conducting power of the air, as appeared in the experiment (N° 4) with raw silk, above mentioned.

If air and water conducted heat in the same *manner*, it is more than probable that their conducting powers might be impaired by the same means; but when I made the experiment with water, by filling the glass globe, in the centre of which the bulb of the thermometer was suspended, with that fluid, and afterwards varied the experiment, by adding 16 grains of raw silk to the water, I did not find that the conducting power of the water was sensibly impaired by the presence of the silk.

But we have just seen that the same silk, mixed with an equal volume of air, diminished its conducting power in a very remarkable degree; consequently, there is great reason to conclude that water and air conduct heat in a *different manner*.

But the following experiment, I think, puts the matter beyond all doubt.

It is well known, that the power which air possesses of holding water in solution is augmented by heat, and diminished by cold, and that, if hot air is saturated with water, and if this air is afterwards cooled, a part of its water is necessarily deposed. I took a cylindrical bottle of very clear transparent glass, about 8 inches in diameter, and 12 inches high, with a short and narrow neck, and suspending a small piece of linen rag, moderately wet, in the middle of it, I plunged it into a large vessel of water, warmed to about 100° of Fahrenheit's thermometer, where I suffered it to remain till the contained air was not only warm, but thoroughly saturated with the moisture which it attracted from the linen rag, the mouth of the bulb being well stopped up during this time with a good cork; this being done, I removed the cork for a moment, to take away the linen rag, and stopping up the bottle again immediately, I took it out of the warm water, and plunged it into a large cylindrical jar, about 12 inches in diameter, and 16 inches high, containing just so much ice-cold water, that, when the bottle was plunged into it, and quite covered by it, the jar was quite full.

As the jar was of very fine transparent glass, as well as the bottle, and as the cold water contained in the jar was perfectly clear, I could see what passed in the bottle most distinctly; and having taken care to place the jar upon a table near the window, in a very favourable light, I set myself to observe the appearances which should take place, with all that anxious expectation which a conviction that the result of the experiment must be decisive, naturally inspired.

I was certain, that the air contained in the bottle could not part with its heat, without at the same time, that is to say, *at the same moment*, and *in the same place*, parting with a proportion of its water; if, therefore, the heat penetrated the mass of air from the centre to the surface, or *passed through it* from particle to particle, in the same manner as it is probable that it passes through water, and all other unelastic fluids, by far the greatest part of the air contained in the bottle would part with its heat, when *not actually in contact with the glass*, and a proportional part of its water being let fall at the same time, and in the *same place*, would necessarily descend in the form of rain; and, though this rain might be too fine to be visible in its descent, yet I was sure I should find it at the bottom of the bottle, if not in visible drops of water, yet in that kind of cloudy covering which cold glass acquires from a contact with hot steam or watery vapour.

But if the particles of air, instead of communicating their heat from one to another, from the centre to the surface of the bottle, each in its turn, and for itself, came to the surface of the bottle, and there deposited its heat, and its water, I concluded that the cloudiness occasioned by this deposit of water would appear all over the bottle, or, at least, not more of it at the bottom than at the sides, but rather less; and this I found to be the case in fact.

The cloudiness first made its appearance upon the sides of the bottle, near the top of it; and from thence it gradually spread itself downwards, till, growing fainter as it descended lower, it was hardly visible at the distance of half an inch from the bottom of the bottle; and upon the bottom itself, which was nearly flat, there was scarcely the smallest appearance of cloudiness.

These appearances, I think, are easy to be accounted for. The air immediately in contact with the glass being cooled, and having deposited a part of its water upon the surface of the glass, at the same time that it communicates to it its heat, slides downwards by the sides of the bottle in consequence of its increased specific gravity, and, taking its place at the bottom of the bottle, forces the whole mass of hot air upwards ; which, in its turn coming to the sides of the bottle, *there* deposites its heat and its water, and afterwards bending its course downwards, this circulation is continued till all the air in the bottle has acquired the exact temperature of the water in the jar.

From hence it is clear why the first appearance of condensed vapour is near the top of the bottle, as also why the greatest collection of vapour is in that part, and that so very small a quantity of it is found nearer the bottom of the bottle.

This experiment confirmed me in an opinion which I had for some time entertained, that, though the particles of air individually, or each for itself, are capable of receiving and *transporting* heat, yet air in a quiescent state, or as a fluid whose parts are at rest with respect to each other, is not capable of conducting it, or giving it a passage ; in short, that heat is incapable of *passing through a mass of air*, penetrating from one particle of it to another, and that it is to this circumstance that its non-conducting power is principally owing.

It is also to this circumstance, in a great measure, that it is owing that its non-conducting power, or its apparent warmth when employed as a covering for confining heat, is so remarkably increased upon being mixed with a small quantity of any very fine, light, solid substance, such as the raw silk, fur, Eider down, &c. in the foregoing experiments : for as I have already observed, though these substances, in the very small quantities in which they were made use of, could hardly have prevented, in any considerable degree, the air from conducting, or giving a *passage* to the heat, had it been capable of passing through it, yet they might very much impede it in the operation of transporting it.

But there is another circumstance which it is necessary to take into the account, and that is the attraction which subsists between air and the bodies above mentioned, and other like substances, constituting natural and artificial clothing. For, though the incapacity of air to give a passage to heat in the manner solid bodies and non-elastic fluids permit it to pass through them, may enable us to account for its warmth under certain circumstances, yet the bare admission of this principle does not seem to be sufficient to account for the very extraordinary degrees of warmth which we find in furs and in feathers, and in various other kinds of natural and artificial clothing ; nor even that which we find in snow ; for if we suppose the particles of air to be at liberty to *carry off* the heat which these bodies are meant to confine, without any other obstruction or hinderance than that arising from their *vis inertiæ*, or the force necessary to put them in motion, it seems probable that the succession of fresh particles of cold air, and the consequent loss of heat, would be much more rapid than we find it to be in fact.

That an attraction, and a very strong one, actually subsists between the particles of air, and the fine hair or furs of beasts, the feathers of birds, wool, &c. appears by the obstinacy with which these substances retain the air which adheres to them, even when immersed in water, and put under the receiver of an air-pump; and that this attraction is essential to the warmth of these bodies, I think is very easy to be demonstrated.

In furs, for instance, the attraction between the particles of air, and the fine hairs in which it is concealed, being greater than the increased elasticity, or repulsion of those particles with regard to each other, arising from the heat communicated to them by the animal body, the air in the fur, though heated, is not easily displaced; and this coat of confined air is the real barrier which defends the animal body from the external cold. This air cannot *carry off* the heat of the animal, because it is itself confined, by its attraction to the hair or fur; and it transmits it with great difficulty, if it transmits it all, as has been abundantly shown by the foregoing experiments.

Hence it appears why those furs which are the finest, longest, and thickest, are likewise the warmest; and how the furs of the beaver, of the otter, and of other like quadrupeds which live much in water, and the feathers of water-fowls, are able to confine the heat of those animals in winter, notwithstanding the extreme coldness and great conducting power of the water in which they swim. The attraction between these substances, and the air which occupies their interstices, is so great, that this air is not dislodged even by the contact of water, but remaining in its place, it defends the body of the animal at the same time from being wet, and from being robbed of its heat by the surrounding cold fluid; and it is possible that the pressure of this fluid upon the covering of air confined in the interstices of the fur, or feathers, may at the same time increase its warmth, or non-conducting power, in such a manner that the animal may not, in fact, lose more heat when in water, than when in air: for we have seen by the foregoing experiments, that, under certain circumstances, the warmth of a covering is increased, by bringing its component parts nearer together, or by increasing its density even at the expence of its thickness. But this point will be further investigated hereafter.

Bears, wolves, foxes, hares, and other like quadrupeds, inhabitants of cold countries, which do not often take the water, have their fur much thicker upon their backs than upon their bellies. The heated air occupying the interstices of the hairs of the animal tending naturally to rise upwards, in consequence of its increased elasticity, would escape with much greater ease from the backs of quadrupeds than from their bellies, had not Providence wisely guarded against this evil by increasing the obstructions in those parts, which entangle it and confine it to the body of the animal. And this, I think, amounts almost to a proof of the principles assumed relative to the manner in which heat is carried off by air, and the causes of the non-conducting power of air, or its apparent warmth, when, being combined with other bodies, it acts as a covering for confining heat.

The snows which cover the surface of the earth in winter, in high latitudes, are doubtless designed by an all-provident Creator as a garment to defend it against the piercing winds from the polar regions, which prevail during the cold season.

These winds, notwithstanding the vast tracts of continent over which they blow, retain their sharpness as long as the ground they pass over is covered with snow ; and it is not till, meeting with the ocean, they acquire, from a contact with its waters, the heat which the snows prevent their acquiring from the earth, that the edge of their coldness is taken off, and they gradually die away and are lost.

The winds are always found to be much colder when the ground is covered with snow than when it is bare, and this extraordinary coldness is vulgarly supposed to be communicated to the air by the snow ; but this is an erroneous opinion ; for these winds are in general much colder than the snow itself.

They retain their coldness, because the snow prevents them from being warmed at the expence of the earth ; and this is a striking proof of the use of the snows in preserving the heat of the earth during the winter, in cold latitudes.

It is remarkable that these winds seldom blow from the poles directly towards the equator, but from the land towards the sea. Upon the eastern coast of North America the cold winds come from the north-west ; but upon the western coast of Europe, they blow from the north-east.

That they should blow towards those parts where they can most easily acquire the heat they are in search of, is not extraordinary ; and that they should gradually cease and die away, upon being warmed by a contact with the waters of the ocean, is likewise agreeable to the nature and causes of their motion ; and if I might be allowed a conjecture respecting the principal use of the seas, or the reason why the proportion of water upon the surface of our globe is so great, compared to that of the land, it is to maintain a more equal temperature in the different climates, by heating or cooling the winds which at certain periods blow from the great continents.

That cold winds actually grow much milder upon passing over the sea, and that hot winds are refreshed by a contact with its waters, is very certain ; and it is equally certain that the winds from the ocean are, in all climates, much more temperate than those which blow from the land.

In the islands of Great Britain and Ireland, there is not the least doubt but the great mildness of the climate is entirely owing to their separation from the neighbouring continent by so large a tract of sea; and in all similar situations, in every part of the globe, similar causes are found to produce similar effects.

The cold north-west winds, which prevail upon the coast of North America during the winter, seldom extend above 100 leagues from the shore, and they are always found to be less violent, and less piercing, as they are further from the land.

These periodical winds from the continents of Europe and North America prevail most towards the end of the month of February, and in the month of March; and I conceive that they contribute very essentially towards bringing on an early spring, and a fruitful summer, particularly when they are very violent in the month of March, and if at that time the ground is well covered with snow. The whole atmosphere of the polar regions being, as it were, transported into the ocean by these winds, is there warmed and saturated with water: and, a great accumulation of air upon the sea being the necessary consequence of the long continuance of these cold winds from the shore, upon their ceasing the warm breezes from the sea necessarily commence, and, spreading themselves upon the land far and wide, assist the returning sun in dismantling the earth of the remains of her winter garment, and in bringing forward into life all the manifold beauties of the new-born year.

This warmed air which comes in from the sea, having acquired its heat from a contact with the ocean, is, of course, saturated with water; and hence the warm showers of April and May, so necessary to a fruitful season.

The ocean may be considered as the great reservoir and equalizer of heat; and its benign influences in preserving a proper temperature in the atmosphere operate in all seasons and in all climates.

The parching winds from the land under the torrid zone are cooled by a contact with its waters, and, in return, the breezes from the sea, which, at certain hours of the day, come in to the shores in almost all hot countries, bring with them refreshment, and, as it were, new life and vigour both to the animal and vegetable creation, fainting and melting under the excessive heats of a burning sun. What a vast tract of country, now the most fertile upon the face of the globe, would be absolutely barren and uninhabitable on account of the excessive heat, were it not for these refreshing sea-breezes? And is it not more than probable, that the extremes of heat and of cold in the different seasons in the temperate and frigid zones would be quite intolerable, were it not for the influence of the ocean in preserving an equability of temperature?

And to these purposes the ocean is wonderfully well adapted, not only on account of the great power of water to absorb heat, and the vast depth and extent of the different seas (which are such that one summer or one winter could hardly be supposed to have any sensible effect in heating or cooling this enormous mass); but also on account of the continual circulation which is carried on in the ocean itself, by means of the currents which prevail in it. The waters under the torrid zone being carried by these currents towards the polar regions, are there cooled by a contact with the cold winds, and, having thus communicated their heat to these inhospitable regions, return towards the equator, carrying with them refreshment for those parching climates.

The wisdom and goodness of Providence have often been called in question with regard to the distribution of land and water upon the surface of our globe, the vast extent of the ocean having been considered as a proof of the little regard that has been paid to man in this distribution. But, the more light we acquire relative to the real constitution of things, and the various uses of the different parts of the visible creation, the less we shall be disposed to indulge ourselves in such frivolous criticisms.

V. *A new Suspension of the Magnetic Needle, intended for the Discovery of minute Quantities of Magnetic Attraction: also an Air Vane of great Sensibility; with new Experiments on the Magnetism of Iron Filings and Brass. By the Rev.* A. Bennet, *F.R.S. Communicated by the Rev. Sir* Richard Kaye, *Bart. F. R. S.*

Read January 26, 1792.

To manifest the various degrees of attraction between magnets and ferruginous bodies, different methods have been used. The substance to be tried has either been simply brought into contact with the magnet, or has been made to float on water or mercury. Needles are commonly made to rest horizontally on sharp-pointed wires, and as an improvement on these methods, Mr. Cavallo has suspended a needle by a chain of horse-hair, consisting of five or six links, which move very freely in each other, and allow the needle to turn more than a whole revolution round its centre. By comparing this needle with others of the best sort in use, he found it much more sensible. Others have suspended the needle by fine threads, or silk: but as these, on turning round a few times, will cause the needle to deviate from its meridian by twisting, they are certainly objectionable.

After considering each of the above methods, and trying some of them in November, 1789, I suspended a small sewing needle, by means of a spider's thread, in the cylindrical glass of my gold-leaf electrometer; and having satisfactorily proved its magnetic sensibility, I now venture to propose this kind of suspension, as being well adapted to experiments requiring the needle to move with the least resistance.

EXPERIMENT I.

From the astonishing tenuity of a spider's thread (see BAKER'S Microscope made easy, Part II. Ch. xxv.), it might be expected that it would bear very much twisting, without causing the needle to be sensibly drawn from its magnetic meridian: but to prove it more fully by direct experiments, I first fastened a small hair to the side of the glass in which the needle was suspended, and placed it so that the point of the needle stood exactly opposite to the point of the hair. Then I turned the needle round, by means of a magnet, about 800 times, and on removing the magnet, I found that the needle rested exactly opposite to the hair: thus a spider's thread only two inches long, by twisting 800 times, did not cause any sensible deviation.

EXPERIMENT II.

A fine harpsichord wire, three inches long, was suspended in a larger glass. This wire was previously rendered magnetic by making it red hot in the flame of a candle, and suffering it to cool in the direction of the magnetic meridian: whereby it acquired polarity by the influence of the earth's magnetic atmosphere alone, and being soft, it possessed (as might be expected) but a weak directive power. The spider's thread was three inches long, and a small hair was fastened by varnish to the north pole of this wire, which served more accurately to distinguish its position opposite to a bit of ivory marked with degrees. This wire was turned round as before, more than a thousand times; yet when suffered to rest, it stood exactly at the same degree, the twist of the spider's thread having produced no sensible deviation.

EXPERIMENT III.

A fine spider's thread was fastened to the spindle of a wheel used for spinning flax; the wheel was placed so that the spindle and thread might hang perpendicularly. To the end of the thread, which was about two inches and an half long, was fastened, by its smaller end, one fibre of the feather of a goose-quill: the lower end of the fibre rested upon a book. The wheel was turned round till the spindle had made above 18,000 revolutions. During this time the spider's thread gradually became about one inch shorter, yet all this twisting did not cause the fibre to turn round when raised from the book. On turning the spindle about 500 times more, the thread broke, apparently by twisting.

Several twisted spider's threads were viewed by means of a very good lucernal microscope, made by Mr. Adams, without being able to perceive any signs of twist, except where it was doubled, and which appeared like hairs growing out of the thread: yet the twist may be distinctly seen by the naked eye, in the sunshine, in some threads to be found amongst trees, where the spider has probably been turned round by the wind during its descent from one branch to another.

EXPERIMENT IV.

A bristle was suspended horizontally by a spider's thread somewhat stronger than the last, and after turning the wheel till it produced 4800 revolutions, it shortened the thread from three inches to one inch; yet either end of the bristle would move towards any warm substance which was presented to it, either with or against the direction of the twist.

EXPERIMENT V.

Several other light substances were suspended by fine spider's threads, and placed in a cylindrical glass about two inches in diameter, as the thinnest part of the wing of a dragon-fly, thistle down, and the down of dandelion; of these, the last appeared most sensible to the influence of heat, for when this down was fastened to one end of a fine gold wire suspended horizontally, or to one end of two bits of straw joined together in the form of the letter T inverted, it would turn towards any person who approached it at the distance of three feet, and would move so rapidly towards wires only heated by my hand, as very much to resemble magnetic attraction. See Tab. II. fig. 1.

EXPERIMENT VI.

A bottle filled with cold water was brought near the glass cylinder, standing in a warm room, and soon after the down of dandelion appeared to be repelled by the bottle, by turning away from it. The bottle was removed to the other side, and the dandelion again moved towards the opposite side.

These attractions and repulsions, since they acted through glass; and at so great a distance, appeared to me at first to be the effects of atmospheres of heat, acting in a similar manner to those of electricity ; and on shewing them to a believer in animal magnetism, he was confident that the light substances were moved by the magnetic atmosphere of the body, and that there was a difference betwixt the attraction of the right hand and the left: but the doubts of my philosophical friends induced me to try the following experiment, which fully explains the matter.

EXPERIMENT VII.

A piece of paper was tied over the mouth of a glass jar about four inches in diameter. Two holes were made in the paper opposite to each other, and near the edge of the glass. The jar was placed upon a table, and suffered to stand a considerable time to cool in a room without fire ; I then sat near it, on the side where one of the holes in the paper was in the nearer, and the other in the farther end of the diameter. I next filled another glass with smoke, and placed it with its mouth over the two holes in the paper. The smoke was now seen to descend through the farthest hole, and mixing with the air in the lower jar, plainly shewed, that the air moved slowly towards the side of the glass warmed by the heat of my body.

EXPERIMENT VIII.

The last experiment proved, that the current of air moving towards, and then ascending up the heated side of the glass, might turn the broader end of the light suspended body, like the vane of a weathercock : but that it might still more decisively appear to be occasioned by a current of heated air, I suspended three inches of gold wire $\frac{1}{280}$ of an inch thick, by a spider's thread; about an inch of the same wire was fastened to the middle, and hanging perpendicularly from the place to which the thread was attached, kept it in an horizontal position whilst undisturbed, and yet suffered it to move up and down like a scale beam, by a small degree of motion in the air. Under one end of this wire heated substances were introduced, which caused it to move upwards, seeming to be repelled by them with as much force, as it was attracted when they were applied horizontally.

Having found that a spider's thread, only two inches and an half long, when twisted by above 18,000 revolutions, would not cause a sensible deviation of the magnetic needle, owing to its very great tenuity, or to its glutinous quality preventing its having any tendency to untwist; and that light substances suspended by it, and inclosed in a glass, were capable of being turned about by so small a degree of heat as that occasioned by a person sitting at the distance of three feet from the instrument; or by wires, or other substances, only warmed by holding in my hand; and that when the instrument was placed in a cool room, a slight touch with the end of my finger would cause the wing of the dragon-fly, or even a bit of straw, to point exactly at the side of the glass which had been touched; there could remain no doubt of the freedom with which a magnetic needle would move when thus suspended: yet another experiment more directly proves its freedom of motion to be greater than that of former methods.

EXPERIMENT IX.

Six rings of horse-hair, made exactly according to Mr.

Cavallo's direction, were suspended in a cylindrical glass jar; to the lowest of these rings a spider's thread, three inches long, was attached. This thread was fastened to a gold wire twisted round the middle of a small sewing needle. The jar was placed with its mouth downwards, and over the edge of a table, the needle hanging a little lower. After the needle and rings of horse-hair were perfectly at rest, the point of the needle was struck with the end of my finger, which caused it to turn round very swiftly, yet this twisting did not move the rings of horse-hair. An harpsichord wire, twenty-one inches long, was suspended by ten spider's threads, to the lowest ring of the horse-hair chain; this was also frequently turned round without moving the rings. A wire of this length was afterwards suspended by spider's threads in a proper frame, and with an ivory scale of degrees, with an intention to observe the daily variation; but it was too much influenced by heat, which I have not yet been able to obviate.

EXPERIMENT X.

To the end of a fine gold wire, three inches long, and suspended by a spider's thread in a cylindrical glass, was fastened a small circular bit of writing paper; light was admitted through a small hole, and also the focus of a large lens was thrown upon the paper, with the intention of observing whether it would be moved by the impulse of light: but though these experiments were often repeated, and once with the paper suspended in an exhausted receiver, yet I could not perceive any motion distinguishable from the effects of heat. Perhaps sensible heat and light may not be caused by the influx or rectilineal projections of fine particles: but by the vibrations made in the universally diffused *caloric* or matter of heat, or fluid of light. I think modern discoveries, especially those of electricity, favour the latter hypothesis.

EXPERIMENT XI.

When cold wires are introduced within the glass jar, and near the light suspended substances, as bits of straw, or very fine wires, a very slight motion of the introduced substances pushes the suspended wires at the distance of an inch, appearing like repulsion, but a contrary motion draws them nearer; therefore it may be attributed to the motion of the air. Also, when a spider's thread, without any thing attached to it, is suspended in a dry glass jar, it recedes from the electric atmosphere of an excited glass tube brought suddenly near the outside of the glass jar, whether the tube be positively or negatively electrified; and when the tube is suddenly removed, the thread is drawn after it. This may be accounted for by the difficulty with which the spider's thread changes its state of electricity, being a very imperfect conductor; whence also, notwithstanding its extreme fineness, it is unfit to be used as an electrometer.

EXPERIMENT XII.

It is indispensibly necessary that a magnetic needle, intended for the discovery of minute quantities of magnetic attraction, should be inclosed in a proper apparatus, that the motion of the air may not disturb it, and that the substances to be tried may be brought sufficiently near, and at right angles with the point of the needle. The following instrument was constructed for this purpose, and is submitted to the approbation of those who are desirous of making further improvements in the science of magnetism. In fig. 2. A is the bottom of the instrument, 5½ inches square, and one inch thick, to lie horizontally on the table, and made of mahogany. B B is a frame of the same wood, standing vertically on the middle of the square bottom, 6½ inches high, and 5½ broad. On one side of the frame a piece of glass is fixed, and a piece of writing paper, gold beater's skin, or other thin substance, is pasted to the edge of the frame on the other side, so that the glass and the paper stand vertically and parallel to each other, at the distance of about half an inch, which is sufficient room for the needle, hanging by a spider's thread from the screw at C, which screw passes through the top piece D, which drops into an opening cut through the frame, and may be taken out by means of the knob of the screw at E. About 10 degrees of a circle are marked upon a bit of ivory fastened to the edge of the frame at F, and the needle, three inches long, is made of the smallest steel harpsichord wire, and is suspended by a spider's thread, also three inches long. A small tapering hair, fastened to the north pole with varnish, and extending about ⅛ of an inch farther than the end of the wire, points at the degrees on the ivory, so that its motion may be very accurately distinguished, and especially by the use of a lens. Round the middle of the needle a small gold wire is twisted, and its end standing perpendicularly is fastened to the spider's thread, to keep the needle in a horizontal position.

The method of obtaining and fastening the spider's thread, which I have used, is this:—I take a forked stick, consisting of a stem and two or three branches, standing at about six inches asunder at their ends, and after finding a proper thread, in the corner of a building, or amongst trees, the ends of the stick are smeared with varnish, and each end applied to the thread, which being turned round the ends of the stick, may be brought away: or several forked sticks may be placed in a room, where spiders are not disturbed, and they will soon attach their threads to them.

When the thread is in this manner extended between the branches, the stick is placed upon the edge of a table, and in the sunshine, if possible, that it may be in a convenient position for attaching it to the needle and screw, by which it is to hang. The gold wire above mentioned is then dipped in varnish, and applied to one end of the spider's thread, so as to entangle it in the varnish, which soon drying, the thread becomes fast, and may be suffered to hang down, the upper end of the thread being yet fast to the forked stick. The point of the screw in the top piece is then dipped in varnish, and the upper end of the thread attached to it in the same manner. Lastly, the needle is gradually lowered through the opening in the top of the instrument, till it hangs opposite to the marked ivory: but if the thread be too long, it is easily shortened, by lifting up the top piece, touching the end of the screw with varnish, and turning it round, so as to wind the thread about it, till it will hang near the edge of the ivory: but to set the point of the hair at the end of the needle at a more exact distance, after the top piece is replaced, the screw is turned by its knob E, which raises or lowers the needle till it hangs right. A second method of raising or lowering the needle, with less trouble in the construction, is to use a small brass wire instead of the screw, which may pass through a cork; or the ivory index may be made to rise and fall by means of a screw. If both sides of the instrument be made of glass, it may be filled with some transparent fluid, which will not cause the metal to rust, as spirit of wine, or oil of turpentine, that the needle may move more steadily, and not be sensibly influenced by heat.

Expedients for rendering the instrument more portable might be easily contrived: but I hope no unprejudiced philosopher will pronounce it useless, because the spider's thread, which is so easily replaced, may break; for sensibility is the only property of essential use in discoveries wherein a small degree of magnetic attraction is to be ascertained.

EXPERIMENT XIII.

The first use I made of my needle, suspended as above, was to try the polarity of several iron utensils; and, as might be expected, they attracted or repelled the north end of the needle, according to their position with respect to the magnetic atmosphere of the earth. A bar of soft iron, half an inch square, and nine inches long, moved the needle very sensibly at the distance of about three feet; longer bars moved it at a much greater distance; and if a bar was held horizontally, near the end of the needle, and at right angles, it might be made either to attract or repel, by moving it up or down only half an inch, so as to appear to change its attraction to repulsion at command; which has surprised persons unacquainted with the importance of position with respect to the earth's magnetic atmosphere. This polarity of position may be very sensibly perceived, by presenting small nails, or smaller bits of wire, above or below the needle, or with the remote end inclining towards the north or south; which plainly demonstrates the existence of a magnetic atmosphere over the earth, where the magnetic fluid being rarefied at one pole, and condensed at the other, occasions the polar direction of the needle, of so much use in navigation.

EXPERIMENT XIV.

At the request of Dr. DARWIN, I repeated an experiment of Mr. CAVALLO, on the increased attraction of iron filings by effervescence with diluted vitriolic acid, inserted in the Philosophical Transactions, Vol. LXXVII. and according to his direction, I placed two ounces of iron filings in an earthen vessel, near the south end of the needle, and first pouring in five parts of water, and then one of good vitriolic acid, a brisk effervescence ensued; but the needle could not be perceived to move. This I repeated six times, sometimes supposing it moved a little, but was not certain, and despaired of success till I recollected of how much importance it was to place the filings in a proper direction as to the earth's magnetic atmosphere; for hitherto, although the filings were placed at right angles with the point of the needle, yet the bottom of the vessel was as much below as the upper surface of the filings was above it. I then took a quantity of filings tied up in a paper, and presenting them to the needle, I found that the bottom always repelled, and the top attracted the north pole; it was therefore necessary to place the bottom of the vessel even with the needle, or rather above it, which circumstance Mr. CAVALLO did not mention. I then placed the vessel at such a distance, that the needle was drawn one degree from its former position; then on adding the water and vitriolic acid as before, the needle came about one degree still nearer; yet it appeared improbable that this increased attraction should be owing to any chemical change in the filings; and as it was so necessary to place them right, it appeared to me likely that whilst each particle of iron was emitting bubbles of inflammable air, and thereby increasing the perpendicular height of the filings, this circumstance would prove the only cause of such increased attraction. To verify this, I put a quantity of filings into an earthen vessel, which covered the bottom an inch deep; then placing them so as to draw the needle two degrees from its meridian, I marked exactly where the vessel stood, and taking it away, I mixed with the filings a quantity of sand, which raised the whole depth of the mixture to two inches; then replacing the vessel, I found the needle drawn above four degrees from its meridian. Still more to confirm this, I placed the filings below instead of above the south pole, and repeated both the effervescence and mixture of sand, and thereby caused as great an increase of repulsion, as of attraction by placing them above. Each of these experiments was repeated, by placing the vessel above and below the north pole also, and the results were accordingly varied: but when small nails were used instead of the filings, no increased attraction was produced, because the nails were too heavy to be raised higher in the vessel, although the effervescence was very violent; whence I was fully convinced that this experiment depended only on raising the perpendicular height of the column of iron filings.

EXPERIMENT XV.

To obviate the suspicion that effervescence might agitate the iron filings, and bring a greater number of them to that side of the vessel which stands contiguous to the magnetic needle, Mr. CAVALLO repeated the experiment with six yards of steel wire twisted in various directions, so as to be admitted into the pot; in which case the metal presented a large surface to the acid, and was not subject to be moved by the effervescence. On adding the diluted vitriolic acid, the needle came somewhat more than half a degree nearer to the pot. After considering the influence of heat in the above experiments, and that the motion of the needle was only half a degree, it may reasonably be suspected to be occasioned by the heat of the vessel: yet I think it may also be accounted for by the change of magnetism in the twisted wire; for, after trying several times to produce this attraction, by pouring the diluted vitriolic acid on a number of bits of wire tied to the end of a glass tube without twisting, I could not produce any increased attraction; at last, by twisting the wire, I succeeded to produce a degree of attraction, which I thought was owing to the expansion or untwisting of the wire during the effervescence. This opinion was strengthened by the changes of polarity I could so easily produce in softened steel wire; first by passing it through a candle, by which it would acquire a very sensible polarity, according to the position in which it was cooled; secondly, by slightly drawing it through my fingers, held in different directions in respect to the earth's polarity; thirdly, when a wire, three inches long, was fastened to the end of a glass tube, and deprived of sensible polarity, by passing through a candle at right angles to the magnetic meridian, it would become possessed of sensible polarity by a small blow with my finger on the glass tube, whilst held in a proper position; fourthly, the smallest degree of pressure between my fingers, so as to bend the wire, would give it polarity. Now, a quantity of harpsichord wire bent in a great number of directions, will possess in some parts a polarity attractive, and in others repulsive, of the south pole of the needle; and when placed in an earthen vessel, with its bottom somewhat above the needle, it attracts the south pole, first by those parts which have acquired a north polarity by bending; and this attraction is strengthened by position; and also position gives north polarity to other parts, in other respects indifferent: then, during the effervescence, the heat of the mixture dilates the wire, and in some places thereby bends it one way or other, which occasions it to acquire the magnetism of position, whence there must sometimes result an increased attraction. From these and other experiments it appears, that whenever the particles of steel (hard enough to be capable of fixed polarity) are by any means displaced, they admit their natural magnetism to become rarefied and condensed by the influence of the earth's magnetic atmosphere; and thus the effect of electrical shocks on steel wires may be explained: but it does not appear probable that the action of acids increases the magnetic attraction of iron, unless accompanied by other circumstances, to which an increased attraction may be more reasonably attributed.

EXPERIMENT XVI.

On reading Mr. CAVALLO's experiments on the increased attraction of iron by effervescence, Dr. DARWIN was led to inquire, whether inflammable air be magnetic. I therefore, at his request, caused inflammable air to issue through a paper tube held near the north and south pole of the needle alternately; the air was also received in a bladder, and applied; but without producing any sensible effect on the needle.

In the LXXVIth Volume of the Philosophical Transactions, Mr. CAVALLO has endeavoured to prove, that brass "does not owe its magnetism to iron, but to some particular "configuration of its component particles, occasioned by the "usual method of hardening it, which is by hammering." Some brass, he observes, will not acquire "any sensible magnetism by hammering." And in other pieces, which have often passed from the workshop to the furnace, and from the latter to the former, there is contained iron, which renders them magnetic.

Now, since some brass is evidently magnetic because it contains iron, it appears to me likely that brass, whose magnetism is made sensible by hammering, contains a smaller quantity of iron, and that hammering renders it sensible, by giving it some degree of polarity. Therefore no brass can acquire this property which contains no iron. This will appear more evident by the following experiments.

EXPERIMENT XVII.

I placed an iron nail, about two inches long, in the fire, where it became red hot, and cooled, as the fire went out, in a position east and west with respect to the magnetic meridian; by which it became very soft, and when presented towards the needle, it attracted or repelled according to its position, having no fixed polarity. The nail was then placed upon an anvil, with the point directed towards the south of the magnetic meridian; and after hammering in this position till it was considerably hardened, its point possessed a fixed south polarity; the other end, being thicker, did not seem to be altered.

Another nail was hammered with its point towards the north, which gave it a fixed north polarity. The polarity of these hammered nails might be instantly changed, by bending the point, whilst held in a contrary position to that in which they were hammered. Several oblong pieces of magnetic brass were hammered in the same manner, and thereby made to possess a north or south polarity, according to their position whilst hammered: hence it appears, that the general effect of hammering is to harden the metal, whereby it becomes in some degree a non-conductor of magnetism, and retains that rarefied and condensed, and therefore more sensible, state of the fluid, which is produced by the influence of the earth's magnetic atmosphere.

EXPERIMENT XVIII.

In a small crucible I placed six thin plates of copper, and betwixt each of them a plate of zinc; these being melted, and cast in a proper mould, produced an oblong piece of brass, which was not sensibly magnetic, nor could I produce any magnetism in it by hammering. The same quantity of copper and zinc were melted, with the addition of some small bits of iron. This brass was very sensibly magnetic, and, when hammered, acquired polarity, by which it more sensibly attracted or repelled the needle. Lastly, a piece of copper was melted, with the addition of some iron, which was also sensibly magnetic. From these experiments I conclude, that brass owes its magnetism to iron; but that it may sometimes contain so small a quantity as not to be sensible till it be hammered.

The greater sensibility, and easy construction, of a magnetic needle, suspended by a spider's thread, as above described, I hope, will contribute to extend magnetic observations, which may in time produce a true theory of this important branch of science ; while the very sensible air vane, from the same mode of suspension, may prevent the apparent attractions and repulsions, occasioned by currents of air, from being confounded with those of magnetism.

Fig. 1.

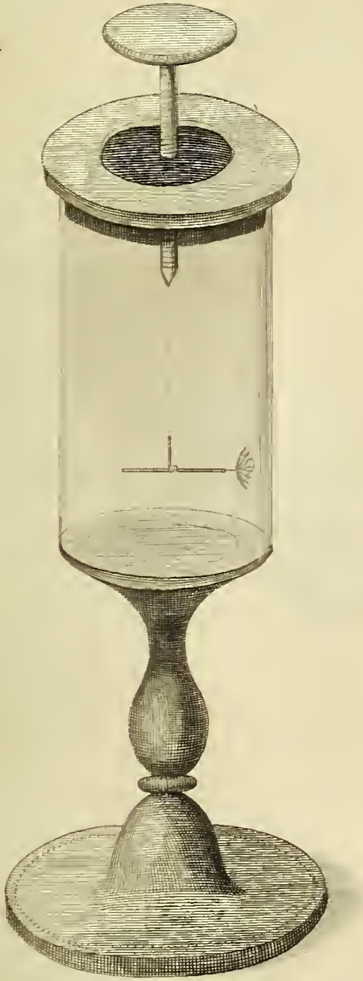
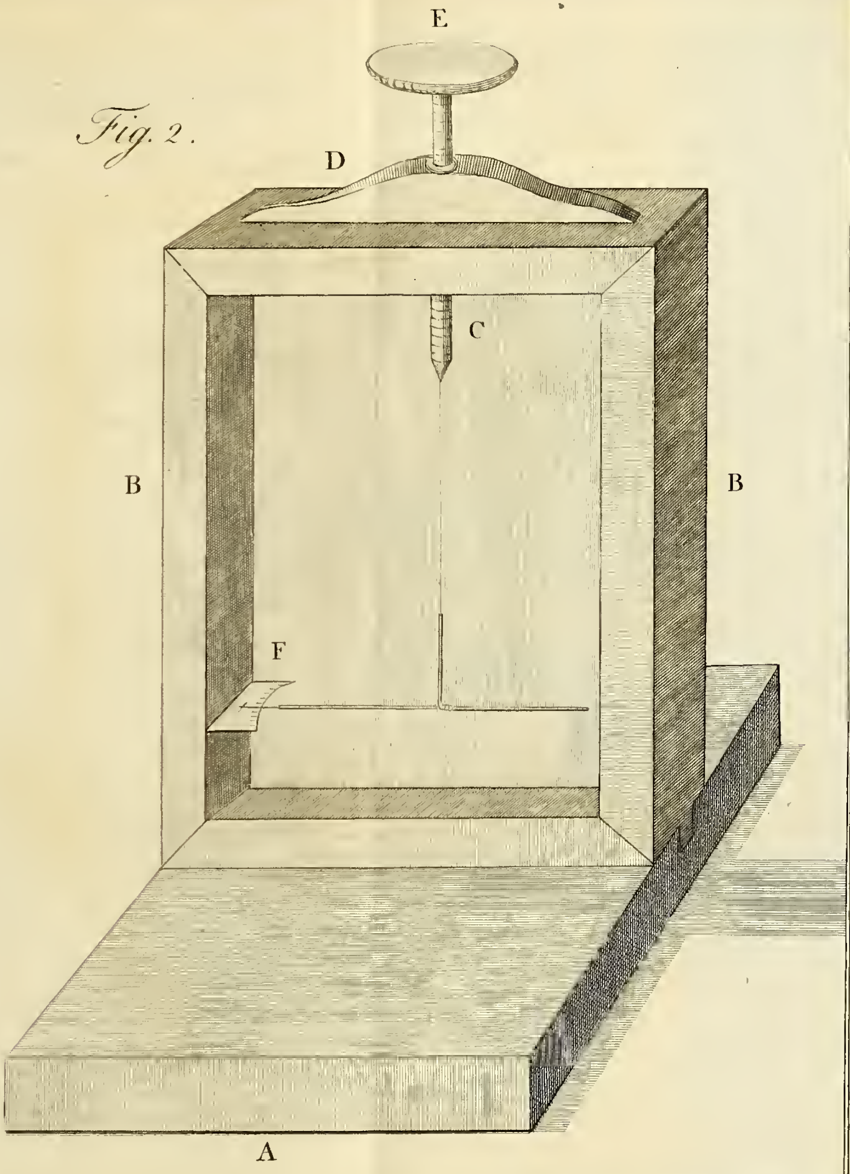


Fig. 2.



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BRITISH MUSEUM NATURAL HISTORY

VI. *Part of a Letter from Mr.* Michael Topping, *to Mr.* Tiberius Cavallo, *F.R.S.*

Read February 16, 1792.

DEAR SIR, *Madras, February* 4, 1789.

I INCLOSE you an account of a base line I have measured for a series of triangles I am carrying down the coast of Coromandel. I have already extended them to about 300 miles from Madras, and am upon returning back to prosecute the work quite down to Cape Comorin. The angles are all taken with my Hadley's sextant, made by STANCLIFFE, by means of three tall signals I have constructed of bamboos, 80 feet high, 60 of which I mount upon steps, so as to see (over all trees, &c.) very distinctly my two other signals, at the distance of from 8 to 13 miles. It is, I believe, the first time the Hadley was ever made use of for a purpose of such magnitude; but it is fully equal to it—nay, it does more—the sun's bearing, or oblique distance, from my signals is also taken by it; by which, and his azimuth (computed) I obtain the angles made by them with the meridian; and by combining the whole, the difference of latitude, and meridional distance of every one of them in English fathoms. This is found so nicely, that a mean of my astronomical observations for the latitudes, never differs more than a few seconds from those given by the geometrical mensuration. In all the operations I have had no one to assist me, except a party of black fellows to carry my flags. I need not tell you how many thousand miles I have travelled to take the angles; nor what the labour and fatigue of such a work must be in this burning climate, where I have frequently had the thermometer at 106° in my tent.

(Signed) M. TOPPING.

An Account of the Measurement of a Base Line *upon the Sea Beach, near* Porto Novo, *on the* Coast of Coromandel, *in May*, 1788; *by Mr.* Michael Topping.

As a necessary foundation for the chain of triangles now carrying on, I have, for some time past, had my thoughts bent towards measuring, with all possible accuracy, a *base line.* This base line, could I have chosen its situation, should have been determined as near the middle of the line of coast I am surveying as possible; but circumstances have not permitted me to make unrestrained choice of its place. On my arrival at Cuddalore, I was told that, as I proceeded southward, I should meet with frequent rivers, and other water courses, that would certainly obstruct me in the design I had formed of measuring it on the sea beach, farther south; and, soon after my removal from that place, I found, with much satisfaction, that the coast, between Cuddalore river and Porto Novo, would serve my purpose extremely well. The beach hereabouts is flat, broad, and remarkably smooth; the only specious objection that can be made to it, is its not being straight, but forming a curved line, concave towards the sea. This, however, I knew to be, in reality, of no bad consequence, since several right lines of sufficient length might, I perceived, be measured upon it; the angles they might interchangeably make, be taken; and the whole afterwards be reduced to one direct line by calculation.

These considerations, and an accident that, about the same time, befel one of my signals, and delayed my trigonometrical progress, finally determined me to measure my base at this opportunity; and I accordingly began that work by placing two of my large signals, at the distance (as nearly as I could judge) of seven miles asunder, for the terms or extremities of it.

In traversing the coast between the signals, I found that, in order to continue my operations throughout upon the most advantageous part of the beach, I must divide the whole distance into six distinct portions, each portion forming a large angle with the next portion to it: and this I immediately did, by placing five lesser flags, at intervals convenient for that purpose.

I now began my measurement, in prosecuting which I spared neither pains nor care. The two rods, of 25 feet each, which I had provided for this use, had been strictly examined while I was at Cuddalore, and their lengths ascertained, as exactly as the conveniences this country affords would admit of. They had been left *purposely* a little too long, as I found it easier to determine, and allow for such excess, than to reduce them to sufficient exactness. In settling their respective lengths, a capital two-feet brass sector, by ADAMS, was made use of, as a standard: this instrument was not only the best authority I could procure; but, being my own property, would, I considered, be ready for an examination, should an opportunity arise of comparing it, in future, with any measure of more acknowledged credit. The thermometer stood at 87°, when the rods were adjusted by it.

The stands, which I had prepared for levelling the rods, were also brought out; and it was with much regret that I found I could not profit by them, as I had hoped to do, assisted as I was by none but black people, in whom, I perceived, it would have been impossible, without incurring great loss of time, to have impressed a necessary idea of their nature and management. I therefore resolved to dispense with the stands, and to lay the rods, end to end, upon the ground. It was in a similar way that the base line for a series of triangles, continued throughout France, was measured. The French rods, which were nearly of the same length and construction with mine, were disposed, in the very same manner, upon the rugged pavement of a highway near Paris; so that I have every reason to believe the opportunity here afforded me, of a peculiarly level and sandy beach, to be the best of the two.

The mode of conducting the measurement was this:—Staves were first set up, in a direct line, between the flags; from every two of these staves a rope was occasionally stretched, as tight as possible, on the ground, and the rods were laid by the side of the rope. The first rod being properly placed, the second was laid *near its end,* and then very carefully adjusted, so as to touch the ferrule of the other, by a man, who had no other employment to engage his attention; and in the performance of this office he was closely watched by myself. The ferrules, which were of thick brass, had been rounded, not only to make the contact more visible, but because the length of each rod was determined, by their having the spherical figure, more easily. At the placing of every *second* rod, which was painted white to distinguish it from the other, I registered its number myself in a book, ruled purposely with columns, each column containing ten numbers: my writer did the same in another book: besides which, an attendant, who was furnished with ten small sticks, gave the Tindal, who also assisted in keeping the reckoning, one of them every time the white rod was laid down; and each man made his separate report to me every tenth number. By these precautions, almost all possibility of a mis-reckoning was prevented; and we accordingly found no disagreement throughout. The whole distance was afterwards re-measured, and gave correctly the same number of rods.

At the conclusion of each several measurement, that I might know exactly where to resume it, a stake, three or four feet long, was driven into the ground, till its head became even with the surface. On the top of this stake the but of the last rod was laid; and a line drawn across it with a pointed instrument, shewed precisely where to re-commence the work. The heads of these stakes served likewise as fixed marks on which, at any time, to place the five smaller flags, or the instrument for taking the requisite angles.

The sum of the six measured lines amounted, by the first trial, to *seven hundred double rods, twenty feet six inches and a half*; and by the second, to *seven hundred double rods, twenty-two feet eleven inches and a half*; their difference being *two feet, four inches and a half*, by which the second measurement exceeded the first. The shortest of these measures is made use of, as operations of this nature have always a tendency to excess, rather than deficiency.

Besides this *linear measurement,* seven essential angles were taken (each angle several times over, for security) with an excellent theodolite by Ramsden. These were the angles formed, at each extremity of the base, by the nearest intermediate flag, and the remote signal; and those formed at each intermediate flag, by the nearest flag to it, on each hand.

The nature and purport of all this will be easily understood, by inspecting the accompanying figure, Tab. III. fig. 1. and attending to the following explanation and

DEMONSTRATION.

Let N and S represent the northern and southern signals, placed at each extremity of the base line NS.

Let ABCDE represent the positions of the five intermediate flags, between each of which, including the two signals, the six right lines, before mentioned, were measured.

At each of these points the angle formed by the flag, or signal, on each side of it, was taken with the theodolite: for instance, at A, the angle formed at that point by the northern signal N, and flag B (177° 24′ 40″); at B, the angle formed by the flags A and C (176° 23′); and so on. At each end of the base line, the angle formed by the nearest flag and the distant signal was also taken; *e. g.* at N, the angle SNA, and at S, the angle NSE.

These things being premised, to find the angles the other four lines make with the base line, proceed in the following manner:—Produce the line NA to K; also the line BA to H; and, through the point A, draw FG parallel to NS, the base line. From the points ABCDE let fall perpendiculars upon the base line, meeting it at OPQR and T.

The angles SNA, NAF, GAK, are all equal; for SNA and NAF are alternate angles, formed by a right line falling upon two parallels (Euclid, 29th, 1st): and NAF, GAK, are equal angles, formed by the mutual intersection of two right lines (Euclid, 15th, 1st). For the same reason, the angle NAH is equal to KAB, and HAF to GAB. Now, the obtuse angle NAB, being known (from observation), its supplement is equal to BAK, which, being subtracted from GAK, leaves the angle GAB required; by means of which, and the hypothenuse AB (given by mensuration), we obtain the side AG of the right-angled triangle GAB, by plane trigonometry; for as rad.: cosine $\angle$GAB : : side AB : side AG. This computed side is equal to the opposite side OP of the rectangular parallelogram OPAG, which being added to the side NO of the right-angled triangle ONA (found by the like analogy from the observed angle N, and the measured hypothenuse NA) gives the distance NP on the base line.

Proceeding in like manner, with the last found angle, to find the angles of the remaining triangles, and thence their respective bases, we shall have the sides of the corresponding rectangles; the sum of which sides, taken together, will be the true measure of the base line NS.

Supposing no imperfection in the instrument with which the angles have been taken, and no error (however small) committed in taking the six preceding angles, then the angle XES, obtained by this method, will prove exactly equal to the angle NSE, which may be had by observation. In the present instance their difference amounts to no more than $2' 5''$.

In the example before us, the observed angles are as follow, from which we may easily deduce the remaining ones required:

Angle at N	5° 36′ 30″	Angle at D	178° 37′ 30″
∠ A	177 24 40	∠ E	177 2 0
∠ B	176 23 0	∠ S	6 53 0
∠ C	178 1 15		

With which proceed thus:

NAB	177°	24′	40″	
Its supplement —	2	35	20	
from	5	36	30	∠ N
gives	3	1	10	∠ required at A.

ABC	176°	23′	0″	
	3	37	0	
—	3	1	10	
	0	35	50	∠ required at B.

BCD		178°	1′	15″
		1	58	45
	+	0	35	50
∠ required at C		2	34	35

CDE		178°	37′	30″
		1	22	30
	+	2	34	15
∠ required at D		3	57	5

DES	177°	2′	0″	
	2	58	0	
+	3	57	5	
	6	55	5	∠ req. at N.

E or S. Diff. only from the observed angle. 2′ 5″

It is immaterial at which end of the base line we begin. In the present case, in order to obtain as great precision as possible, the intermediate angles have been deduced from both ends; those at A and B, from the observation at N; and the remaining ones at C D and E, from that at S. Had, however, this precaution been neglected, the error induced, by deriving the four required angles from either primitive, at N or S, would not have affected the true length of the whole base line more than 0,27 of a foot, or not quite so much as three inches and a quarter.

This method of obtaining the measure of an inaccessible line, where the measured lines every where make small angles with it, is a very accurate one; for though, in oblique triangles, small angles, from the difficulty of taking angles perfectly, are likely to produce considerable errors, in right-angled triangles it is the very reverse; as in them the smaller the angle taken, the more accurate will be the result.

Fig. 1. is not drawn in due proportion, either as to the sides or angles; as, had it been justly delineated, there would not have been sufficient room for the auxiliary lines, and letters of reference. Fig. 2. is protracted by scale and sector, and shews the relative length and bearing of the several measured lines, as well as the direct distance of their farthest extremities, and their position with respect to the meridian.

For distinction sake, the measured hypothenusal lines are drawn thicker than those about them; and the lines which are parallels to the base line are dotted.

Table of the measures, with their corrections, of the six hypothenusal lines.

Inch. Inch.
Note, the error of the rod A was 0,10+, of B 0,12+.

Distances and number of double rods between the stations.				Measured lengths in feet, &c.			Corrections; subtractive.			Correct length of each hypothenuse.		
	D. Rods.	Feet.	Inches.	Feet.	Inch.	Dec.	Feet.	Inch.	Dec.	Feet.	Inch.	Dec.
Between N signal and 1st flag	146	9	4	7309	4	0	2	8	1	7306	7	9
Between 1st and 2d flags	170	37	6	8537	6	0	3	1	5	8534	4	5
Between 2d and 3d flags	106	11	6	5311	6	0	1	11	4	5309	6	6
Between 3d and 4th flags	83	10	3	4160	3	0	1	6	4	4158	8	6
Between 4th and 5th flags	93	8	2	4658	2	0	1	8	5	4656	5	5
Between 5th flag and south. signal	100	43	5,7	5043	5	7	1	10	2	5041	7	5
							Total effect of error,					
							12	10	1+			

Table for computing the R. A. Triangles, and reducing the six measured lines to one rectilinear distance.

Angular points.	Angles.	Correct Hypothenuses.	Logarithms.	Reductions.
At north. signal N	5° 36′ 30″ W$^{ly.}$	1st Hypothenuse, Feet. Dec. 7306, 66 NA	9. 9979161 3. 8637189 3. 8616350	Feet. Dec. 7271 68 NO
At 1st flag A	3 1 10 W$^{ly.}$	2d Hypothenuse, 8534, 38 AB	9. 9993967 3. 9311720 3. 9305687	8522 53 OP
At 2d flag B	0 35 30 E$^{ly.}$	3d Hypothenuse, 5309, 55 BC	9. 9999764 3. 7250577 3. 7250341	5309 26 PQ
At 3d flag C	2 32 30 E$^{ly.}$	4th Hypothenuse, 4158, 72 CD	9. 9995725 3. 6189596 3. 6185321	4154 63 QR
At 4th flag D	3 55 0 E$^{ly.}$	5th Hypothenuse, 4656, 46 DE	9. 9989845 3. 6680559 3. 6670404	4645 59 RT
At 5th flag. E and south. sig. S	6 53 0 E$^{ly.}$	6th Hypothenuse, 5041, 63 ES	9. 9968584 3. 7025710 3. 6994294	5005 29 TS 34908 98 NS

Or 11636 English yards (rejecting the Decimals), or 6 Miles. 4 F. 196 Yards. total length of the *base*.

Observations taken, both with the theodolite, and the Hadley, for determining the position of the base line, with respect to the meridian.

1788.	App. time.	Distance of ☉'s centre from signal observed.	☉'s altitude, centre cor.	☉'s azimuth computed.	Horizontal ∠ between ☉'s centre and signal.	Angle made by signal with meridian.
May	H. ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″
24	about 7 A. M.	—	21 52 50	71 59 18	With theodolite, mean of 6 sights, 111 34 0	3 33 18
25	about 7 A. M.	—	19 22 37	71 32 1	mean of 6 68 3 35	3 28 26
27	about 5 P. M.	—	18 45 54	71 3 40	mean of 6 105 26 35	3 29 46
29	about 7½ A. M.	—	29 20 18	71 29 20	mean of 6 111 56 8	3 25 28
29	about 4½ P. M.	—	27 45 41	71 21 34	mean of 6 105 9 28	3 29 6
					mean	3 29 12
29	A. M. 7 18 26	mean of 4 Hadley obs. 110 35 30	computed 22 19 26	71 6 30	computed 112 21 10	3 27 40
29	P. M. 4 27 39	mean of 3 103 42 9	computed 25 35 0	71 17 40	computed 105 13 47	3 28 33
					mean	3 28 6

The mean of 28 observations by the theodolite is - 3° 29′ 12″

The mean of 7 observations with the Hadley is - - 3 28 6

The medium of both is therefore (by which the south end of the base is westerly, and the north end easterly of the meridian) - - - 3 28 39

N. B. The set of observations, taken as above, on the 25th of May, were made at the southern signal; all the rest at the northern signal.

Meridional observations of stars for determining the latitude of each end of the base line.

Stars.	True zenith at south end of base.	Distances at next southern station.	Latitude of south end of base.
β in Centaur -	70° 54′ 3″	—	11° 33′ 34″
α in Centaur -	71 31 19	—	11 33 48
Antares - -	37 30 14	—	11 33 40
α in Centaur -	—	71 27 4	11 33 34
β in Cross - -	—	70 0 50	11 33 2
η in Ursa Major	—	38 53 16	11 33 14
β in Centaur -	—	70 49 11	11 32 43
Antares - -	—	37 25 58	11 33 25
η in Ursa Major	—	38 53 1	11 33 29
β in Centaur -	—	70 50 11	11 33 43
α in Centaur -	—	71 26 58	11 33 27
η in Ursa Major	—	38 53 16	11 33 14
β in Centaur -	—	70 49 26	11 32 58
Mean latitude of south end -			11 33 22
			+ 5 42
Latitude of north end of base line			11 39 4

To settle the position of the base, with respect to the meridian, the sun's bearing from each signal, and his azimuth, were observed several times, both with the theodolite and the Hadley; and, having made the necessary computations, I became satisfied of the justness of an opinion I had before entertained, that the Hadley is by far the best instrument in general practice for such purposes: for though the theodolite has the advantage, from its fixed position, and the power of its telescope, in taking horizontal angles *upon the horizon*; yet, at any considerable elevation, when a strict attention is required to the vertical adjustments of the theodolite, such attention is incompatible with the nature of a portable instrument, which is ever liable to suffer change in its adjustments by even the most careful removal from place to place.

The table, page 110, gives the results of these observations, and the data from which they were computed. Nothing need be said of the two preceding tables, which sufficiently explain themselves.

The last table contains meridional observations of stars for determining the latitude of each respective termination of the base line. No observations were obtained at the northern extremity, the weather proving uncommonly cloudy while I was there; and only three latitudes were observed at the south end. The others were taken at my next southern station, and reduced to the terms of the base by the adjacent triangles. The difference of latitude between that station and the south end of the base, I found by measurement to be 8201 yards, or 4′ 1″; and the difference of latitude between the terms of the base 5′ 42″. For considering a portion of the meridian passing through either end of the base, as the cosine of an arc, whose radius is 11636 yards, and subtense 3° 28′ 39″, we shall find the difference of latitude to be 11614 yards = 5′ 42″. By a mean of all the astronomical observations, the south end of the base lies in latitude 11° 33′ 22″ N.; the north end consequently lies in latitude 11° 39′ 4″.

Having completed the measurement and principal calculations, I caused a large stone to be placed at each extremity of the base, to mark and perpetuate it for future occasions. The stone at the north end I inscribed with the following characters:

S. 3° 29′ W.
Yds.
11636.
T.
1788.

And that at the south end as follows:

N. 3° 29′. E
Yds.
11636.
T.
1788.

both inscriptions equally implying, that the opposite end of the base line lies in the direction therein expressed, distant 11636 English yards from the stone so inscribed.

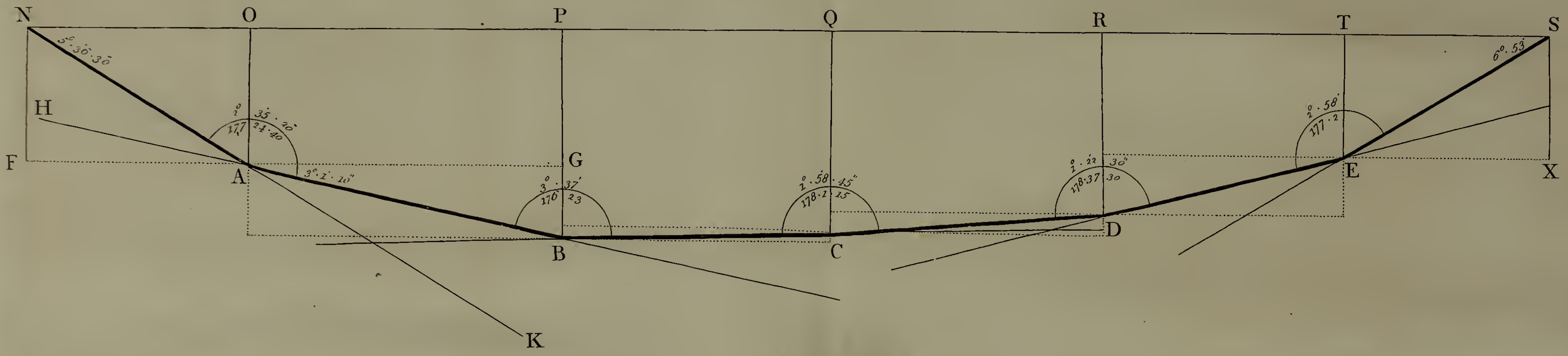
It may be proper to record, that the north end of the base is on the sea beach, 27 yards from the surf; and 129 yards N. E. by E. easterly, from an old choultry in a Palmyra tope, called Namaswaumoodaly Choultry. From the mouth of Cuddalore river to the north end of the base I measured, with a perambulator, just four miles and one furlong.

The south end of the base lies 42 yards from the surf, and 169 yards E. $\frac{1}{2}$ S. from a decayed choultry in a Palmyra tope, called Chinnachitty Choultry. There is another choultry and tope near the centre of the base; but the northern cluster of trees before mentioned, is the first cluster we meet with, by the sea side, southward of Cuddalore river.

This measured base makes the exterior side of the thirty-third triangle of a concatenated series of oblique triangles, now carrying down the coast from the steeple of Fort Saint George.

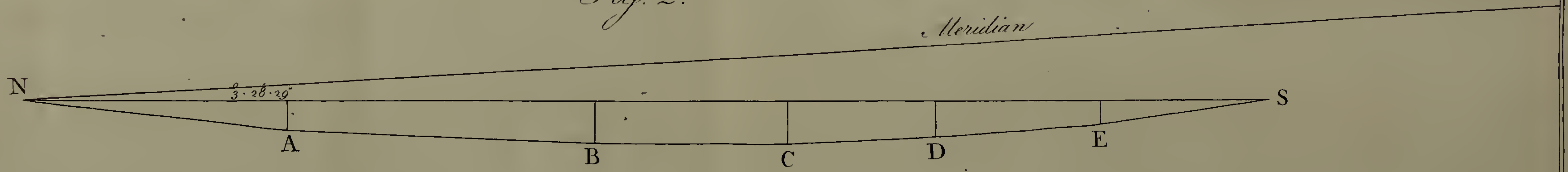
Porto Novo,
10th June, 1788.

Fig. 1.



Scale 4000 Feet to an Inch

Fig. 2.



Basire sc.

SEUM
ISTORY.

VII. *Description of* Kilburn Wells, *and Analysis of their Water. By Mr.* Joh. Godfr. Schmeisser. *Communicated by Sir* Joseph Banks, *Bart. P.R.S.*

Read February 23, 1792.

THOSE wells lie to the right of the Edgeware Road, about two miles from London, in a dry, but verdant, and gently rising meadow.

They spring about 12 feet below the surface, and are covered with a small stone cupola.

The diameter of the well near the surface of the water is about five feet; the depth of the water was in July and August two feet; this its general depth increases in winter, at times, to three feet; the changes in the atmosphere do not appear to affect either the quantity or quality of the water.

This mineral water is not perfectly bright, but of rather a milky hue; it has a mild and bitterish taste, with little or no briskness, as containing a very small proportion of fixed air. On dipping for the water, or otherwise agitating it, a sulphureous smell is perceived near the surface; which, however, soon goes off in a temperature of about 80 degrees of FAHRENHEIT'S thermometer, *which I have used throughout*.

The specific gravity of the Kilburn water is to distilled water as 1,0071 : 1,0000; its general temperature 53°, which was not affected by a change of 10 degrees in the temperature of the atmosphere. While the water continues at rest, no bullition of fixed air is perceived, and scarce any sulphureous smell.

That this mineral water so easily parts with the hepatic air (perceivable on agitating it) if it be shaken in a warmer temperature, or transported from one place to another, is probably owing to the fixed air which it contains: for as this aerial acid has a great affinity to phlogiston; so it may hence be inferred, that fixed and hepatic air cannot exist together in a mineral water, but that the latter will be destroyed, as the fixed air is developed by gentle warmth.

Chemical Experiments. Examination of the Kilburn *Waters by reagent Substances.*

EXPERIMENT I.

The tincture of litmus is a very little affected by the water fresh from the spring, and not at all after having been boiled; which proves that this water contains very little aerial acid.

EXPERIMENT II.

Paper stained with a decoction of logwood is somewhat changed, to rather a bluish hue, by fresh water; from this I infer, that the water contains a little absorbent earth dissolved in aerial acid.

EXPERIMENT III.

Paper stained with turmeric is not changed by this water; which would happen if it contained any uncombined alkaline salt.

EXPERIMENT IV.

On adding 42 drops of the purest concentrated vitriolic acid to two pounds of the Kilburn water, it became perfectly clear, and some air was disengaged; this air rendered lime-water turbid.

EXPERIMENT.—A few drops of pure and nitrous acid were dropped into a tumbler full of the water; the smell of hepatic air was diminished, and hardly any precipitate formed. From this experiment it becomes probable, that the water contains no liver of sulphur, but only hepatic air: from the appearances on adding the vitriolic acid, may be inferred, that this water contains little calcareous earth, and no *terra ponderosa*.

EXPERIMENT V.

In order to ascertain whether the hepatic air really existed in the water, or whether the appearances which made this probable might not arise from the air of marshes, which will occasionally imitate the other, I filled three quart bottles with distilled water, and nearly emptied them just over and almost in contact with the spring. The air, which of course took the place of the water I had emptied, was subjected to the following experiments:

(a) A piece of white arsenic being immersed in it, its surface soon became yellow.

(b) A solution of lead being put into one of these bottles, the precipitate which was formed soon became of a blackish brown colour.

(c) A solution of silver being put into the third bottle, the precipitate formed was blackish. All these are proofs of the existence of the hepatic air.

EXPERIMENT VI.

On adding a few drops, both of the aqueous and spirituous infusion of galls, to a glass of the fresh water, no change of colour took place; yet by means of well saturated phlogisticated alkali some traces of iron were perceived.

EXPERIMENT VII.

A solution of soap in spirit of wine being dropped into the water, was immediately decomposed by it. This proves it to contain neutral salts.

EXPERIMENT VIII.

By adding some acid of sugar both to the fresh and the boiled Kilburn water, calcareous earth was precipitated; which shows that this water contains aerated calcareous earth, and selenite.

EXPERIMENT IX.

By adding aerated volatile alkali, magnesia and calcareous earth were precipitated both from the fresh and the boiled water.

EXPERIMENT X.

Caustic volatile alkali precipitated both magnesia and calcareous earth from the fresh water; a proof of its containing these earths in a state of combination with the aerial and other acids.

EXPERIMENT XI.

Caustic fixed alkali precipitated from the fresh water aerated magnesia. (It is to be observed with regard to this experiment, that the precipitate should be separated as soon as formed; otherwise, if there be iron contained, that will also be precipitated together with calcareous earth).

EXPERIMENT XII.

On dropping muriated *terra ponderosa* into the fresh water, the earth is precipitated; which also happens, but in a less degree, if the water has been boiled; this proves it to contain vitriolated soda and magnesia, the other selenite.

To ascertain the quantity of vitriolic acid contained in these salts, as much pure acetous acid was first added to one pound of the water, as was required to saturate the earth. Then a solution of *terra ponderosa* in nitrous acid was carefully dropped into the mixture, till no more precipitate was formed; the thus regenerated spar was carefully collected, edulcorated, and dried, when it weighed 60 grains. Now, if 100 grains of ponderous spar contain 22 grains of vitriolic acid, it will follow, that one pound of the Kilburn water contains about 13 grains of this acid.

EXPERIMENT XIII.

Vitriolated silver was dissolved, and added to the Kilburn water, previously impregnated with pure nitrous acid, to effect a solution of the earthy particles contained in it. The silver combined with the muriatic acid in the water, and formed a *luna cornea*: but I do not estimate the quantity of the acid in the water from this experiment, which is liable to deceive, as well as the preceding. This will appear on comparing the result with the real quantity of vitriolic acid, as given in the contents of the water, annexed to these experiments.

EXPERIMENT XIV.

A quantity of the Kilburn water having been gently evaporated to dryness, a powder remained; some of this being triturated with vegetable alkali, there was no smell of volatile alkali perceived.

EXPERIMENT XV.

A little of the powder having been mixed with tartar, and thrown into a red hot crucible, no detonation happened; of course nitre was not one of the constituent parts.

EXPERIMENT XVI.

On moistening a little of the powder with pure and concentrated vitriolic acid, there arose muriatic vapours; a proof there were no salts formed with the nitrous acid existing in this water.

The Kilburn water therefore contains fixed air, hepatic air, earthy neutral salts, vitriolated and muriated neutral salts, calcareous earth, magnesia, selenite, and a very little iron.

These component parts of this mineral water appeared on the addition of reacting substances; and with this guide I proceed to the analysis. I beg, however, first to mention another experiment or two, relative to the effects of this water. Two quarts of the fresh drawn water having been successively drank, operated gently downwards, but at the same time affected the head a little. This species of intoxication was, however, not produced, if the water had been freed from its hepatic air. The celerity of the pulse was but little increased by it; yet the following experiment will prove that it pervaded the whole system, and afford a very strong argument in favour of its efficacy. A small plate of silver was placed under the arm, in contact with the skin, and thus worn for some hours without being tarnished by the perspirable matter ; a bottle of the Kilburn water having now been drank, in less than half an hour the silver was become black.

One ounce of fresh gall was mixed with a quart of the water as it came from the spring, and into another bottle was put the same quantity of gall, with a quart of distilled water, and both were placed in a warmth of 96°. After ten hours, the latter mixture began to show signs of putrefaction, while that with the Kilburn water continued perfectly sweet. Twelve hours after, this also became putrid.

Two ounces of very putrid gall were mixed with a quart of Kilburn water, and placed in the same warmth. The fœtor was soon diminished, and after three hours no longer perceptible.

Two ounces of putrid gall were mixed with two ounces of distilled water, in which 24 grains of the saline mass, obtained by evaporation from the Kilburn water, had been previously dissolved ; and this mixture was likewise placed in a warmth equal to 96°. After $2\frac{1}{2}$ hours the offensive smell had gone completely off.

Similar experiments were made with blood, and the results were the same.

Experiments to ascertain the Properties and Proportion of the Elastic Fluids, contained in the Kilburn *Water.*

The apparatus with which these experiments were instituted contained 16 cubic inches: the cylinder for the reception of the extracted air, eight cubic inches. Into the jar were put 14 cubic inches of the fresh drawn water ; and having been immediately connected with the apparatus, it was placed in a lamp furnace. The heat having been gradually increased till the water began to boil, six cubic inches of the quicksilver, with which the cylinder had been previously filled, were displaced by the air which came over. This vessel having been artificially cooled to 53°, the air was contracted one-fourth of an inch. Now, if the two cubic inches of atmospheric air, left in the jar, be deducted, there remain $3\frac{3}{4}$ cubic inches of air expelled from 14 inches of the water. By agitation in lime-water $2\frac{1}{4}$ cubic inches were absorbed, the precipitated aerated lime weighed $2\frac{1}{8}$ grains. The remaining gas was found to be hepatic air.

Experiments to ascertain the fixed constituent Parts of the Kilburn *Water, and their Properties.*

EXPERIMENT I.

Twenty-four pounds of water (at 16 ounces) were evaporated in one of the Wedgwood basons, by a gentle heat, down to four ounces; this residuum was then reduced to perfect dryness in a small glass vessel. The mass thus obtained was scaly, with crystals intermixed, and of a yellowish hue; its taste was bitter, and but little sharp; the weight $1900\frac{100}{197}$ grains (which were equal to 1560 grains of the further used accurate weight), which divided by 24, gives for every pound of the water 79 grains of solid matter. The bason in which the first evaporation had been made, was rinced with a little *aqua regia*, that such earthy particles as might have adhered to it should not be lost; this solution was put aside and marked A.

EXPERIMENT II.

The dry residuum obtained as above was rubbed with a little alcohol, and a sufficiency of this spirit having been added, the whole was placed in a gentle warmth, and often stirred with a glass tube: after a few days the fluid was decanted, and what remained indissoluble, having been edulcorated with alcohol, and carefully collected and dried in a moderate warmth, was found to weigh 1392 grains; so that 168 grains had been taken up by the alcohol.

The spirituous solution was gently evaporated, when 180 grains of a yellowish, easily deliquescent salt remained, having a bitter and acrid taste. By adding to this one ounce of the strongest alcohol, all the deliquescent salts were dissolved, leaving 40 grains of a saline substance; which having been again dissolved in distilled water and crystallized, was found to be common salt.

The solution of the deliquescent salts having been mixed with 20 drops of pure vitriolic acid, some selenite appeared; the whole was now evaporated to dryness, and having been mixed with $\frac{3}{5}$ of its weight of pure vitriolic acid, it was exposed to a considerable heat. The vapours thus expelled were those of the muriatic acid. When these had ceased, and the mass was cold, I again dissolved it in distilled water, when a black flaky substance was separated, which being carefully collected on filtering paper, and dried, weighed six grains; this was resinous matter.

The solution was now placed on the fire, in order to evaporation, during which 12 grains of selenite were separated; the remainder afforded vitriolated magnesia, leaving some drops of a yellowish fluid, from which, by the addition of caustic volatile alkali, a very small quantity of calx of iron was precipitated, which, when dry, weighed about $\frac{1}{8}$ of a grain.

It appears, from the above experiments, that the spirituous solution held of

Muriated soda - -	40 grains.
——— magnesia - - -	128 grains.
——— calcareous earth -	6 grains.
Resinous matter - -	6 grains.
Calx of iron - -	$\frac{1}{8}$ grain.

EXPERIMENT III.

The residuum of the second experiment, which was not soluble in alcohol, was digested with distilled water, and often stirred. The water took up 1204 grains. This solution was filtered, the residuum often edulcorated and dried; this weighed 188 grains. The solution was evaporated in a gentle warmth to one third, and being then set in a cold place, 12 grains of selenite were separated. Having further evaporated the remaining solution, I now mixed it with double its weight of alcohol, and after having again heated this mixture, I let it cool gradually; thus all the vitriolic salts were separated. I again dissolved this saline mass in distilled water, and, after gentle evaporation, obtained crystals, weighing altogether 1200 grains, and consisting of vitriolated tartar, and vitriolated soda: from the remaining ley I obtained, on further evaporation, 10 grains more of common salt. There were no traces of an uncombined alkali, which must otherwise have now shewn itself. The 1200 grains of mixed salts, which had crystallized first, were again dissolved in water, and this solution made to boil; a hot solution of mineral alkali was now mixed with it, and the magnesia thus separated weighed, when washed and dried, 170 grains: this having been again saturated with diluted vitriolic acid, afforded 910 grains of pure crystallized vitriolated magnesia. The solution remaining after the separation of the magnesia having been duly evaporated, yielded 282 grains of crystallized GLAUBER salts, exclusive of what had been formed by the above-mentioned admixture of the natron.

EXPERIMENT IV.

The 188 grains of remaining earth, mentioned in the preceding experiment, were put into *aqua regia*, and the solution mentioned in the first experiment, as marked (A), was added. This mixture having been well heated, and again suffered to cool, was put on some filtering paper; and what remained on this, having been well washed with diluted spirit of wine, and dried, weighed 112 grains.

The filtered solution was then gently evaporated, during which it deposited six grains of selenite; a sufficiency of phlogisticated alkali was now added, to separate the iron; 25 grains were required, and the dried blue precipitate weighed 15 grains. (It is to be observed, that the phlogisticated alkali contained, in 25 grains, 4½ grains of the calx of iron.)

The above blue precipitate was put into a small crucible, and kept for a proper time in a red heat, when it left 7½ grains of calx of iron, which was attracted by the magnet; if from this be deducted the 4½ grains contained in the alkali, there will remain three grains which had been contained in the water.

EXPERIMENT V.

The solution, from which I had precipitated the iron, was evaporated, and mixed with vitriolic acid, and diluted spirit of wine, when 72 grains of selenite were separated: these 72 grains of selenite having been boiled with mineral alkali, yielded 24 grains of calcareous earth.

EXPERIMENT VI.

The remainder of the solution from which the calcareous earth had, by means of the vitriolic acid, been separated in the form of selenite, yielded, by adding mineral alkali, 72½ grains more of magnesia. All the selenite obtained was boiled in 1200 times its weight of water, in which it was completely dissolved, no siliceous earth being left.

As a proof that the process had been properly conducted, I saturated both the obtained earths with diluted vitriolic acid; when the first again afforded selenite, and the other vitriolated magnesia.

Summary of the constituent parts of the Kilburn *Water, in* 24 *Pounds.*

Fixed air		84	cubic inches
Hepatic air	near	36	
Vitriolated magnesia		910	grains, equal to ℥ij ʒiiss, Apothecary's weight.
Vitriolated natron		282 gr.	= ʒv. lij grains
Muriated natron		60 gr.	= 75 gr.
Selenite		130 gr.	= ʒ ij xlij gr.
Muriated magnesia		128 gr.	= ʒij xl gr.
——— calcareous earth		6 gr.	= 7½ gr.
Aerated magnesia		12½ gr.	= 15 gr.
——— calcareous earth		24 gr.	= 30 gr.
Calx of iron		3⅛ gr.	= 4 gr.
Resinous matter		6 gr.	= 7½ gr.

Sum 1561⅝ grains, equal to medicinal weight 4 ounces, 0 drams, and 32 grains.

VIII. *Observations on Bees.* *By* John Hunter, *Esq. F.R.S.*

Read February 23, 1792.

Of the Common Bee.

The common bee, from a number of peculiarities in its œconomy, has called forth the attention of the curious; and, from the profit arising from its labours, it has become the object of the interested; therefore, no wonder it has excited universal attention, even from the savage to the most civilized people: but it has hardly been considered by the anatomist; at least the two modes of investigation have not gone so much hand in hand, as they ought to have done.

The history of the bee has rather been considered as a fit subject for the curious at large, whence more has been conceived, than observed. Swammerdam, indeed, has rather erred on the other side, having, with great industry, been very minute on the particular structure of the bee. I shall here observe, that it is commonly not only unnecessary to be minute in our description of parts in natural history, but in general improper. It is unnecessary, when it does not apply to any thing, but the thing itself, more especially if it be of no consequence; but whenever it applies, then it should so far be treated accurately. Minutiæ beyond what is essential, tire the mind, and render that which should entertain along with instruction, heavy and disagreeable; the more so too, if the parts are small, where the sense can only take them in singly, and the mind can hardly comprehend the whole, or apply all the parts combined to any consequent action. This has been too much the case with SWAMMERDAM; he often attempted too much accuracy in his description of minute things. But the natural history of insects has not been sufficiently understood at large, so as to throw light on this subject where there was an analogy, and where, without such analogy, it must appear in the bee alone unintelligible, from the obscurity attending some parts of their œconomy; for there is hardly any species of animals but what has some of its œconomy obscure; and probably this is as much so in this insect, as in any other class of animals we are at one season of the year almost daily seeing; yet these parts of the œconomy may be evident in some other species of the same tribe or genus, and thus be cleared up, from analogy, so that the species assist each other in their demonstration. This is evident in the whole tribe of flying insects, for what is lost, or cannot be made out in the one, may be demonstrated in another: and we find there are some things in the œconomy of the bee that cannot be seen or demonstrated in it alone, but which are evident in some other insects; and while they possess the same parts, and other circumstances are similar, we must conclude the uses of those parts are similar in both; for whenever a circumstance in one animal cannot be found out in that animal, but can in another, then the natural conclusion is, that the uses are similar in both.

Though the bee may be classed in some degree among the domestic animals, yet from there being such a cluster of them, and because they are an offensive and irritable animal, their actions are rendered very obscure, and can only be observed by little starts; often we can only see the effects, which renders the knowledge of their œconomy still imperfect; they would in many cases seem to evade our wishes; they often remove out of our sight part of their œconomy, when they can. Thus they often remove their eggs and young. Many quadrupeds do this, as cats, &c. and I have reason to believe, that birds can remove their eggs, at least I have reason to suspect the sparrow of this.

As the bee is an insect, it has most things peculiar to that class of animals: such as are common are not to be taken notice of in the history of this insect, but only its peculiarities which distinguish it from all others, and constitute it to be a bee; and as bees form a large tribe of insects, it is the more singular peculiarities that constitute a distinct species of this tribe. As most parts of the œconomy of insects have not been in every respect understood, and although now known in some insects, yet cannot be observed in the bee, but which accord with many circumstances attending this insect, therefore such must be brought into the present history of the bee, to render it more complete. I shall not be minute in the anatomy of this animal, as that would be too tedious and uninteresting. When we talk of the œconomy of the colony, such as the secreting wax, making combs, collecting farina, honey, feeding the maggots, covering in the chrysalis, and the honey, stinging, &c.; it is the labouring bees that are meant. In pursuing any subject, most things come to light as it were by accident; that is, many things arise out of investigation that were not at first conceived, and even misfortunes in experiments have brought things to our knowledge that were not, and probably could not have been previously conceived: on the other hand, I have often devised experiments by the fireside, or in my carriage, and have also conceived the result; but when I tried the experiment, the result was different; or I found that the experiment could not be attended with all the circumstances that were suggested.

As bees, from their numbers, hide very much their operations, it is necessary to have such contrivances as will explore their œconomy. Hives, with glass lights in them, often shew some of their operations, and when wholly of glass, still more; but as they form such a cluster, and begin their comb in the centre, little can be seen till their work becomes enlarged, and, by that time, they have produced a much larger quantity of bees, so as still to obscure their progress. Very thin glass hives are the best calculated for exposing their operations; the distance from side to side about three inches; of a height and length sufficient for a swarm of bees to complete one summer's work in. As one perpendicular comb, the whole length and height of the hive, in the centre, dividing it into two, is the best position for exposing their operations, it is necessary to give them a lead or direction to form it so; therefore it is proper to make a ridge along the top from end to end, in the centre, between the two sides, for they like to begin their comb from an eminence; if we wished to have them transverse, or oblique, it would only be necessary to make transverse, or oblique ridges in the hive. I had one made of two broad pieces of plate-glass, with glass ends, which answered for simple exposure very well; but I often saw operations going on, when I wished to have caught some of the bees, or to take out a piece of comb,

&c.; therefore I had hives made of the same shape and size, but with different panes of glass, each pane opening with hinges, so that if I saw any thing going on that I wished to examine more minutely or immediately, I opened the pane at this part, and executed what I wished, as much as was in my power; this I was obliged to do with great caution, as often the comb was fastened to the glass at this part. When I saw some operations going on, the dates or periods of which I wished to ascertain, such as the time of laying eggs, of hatching, &c. I made a little dot with white paint opposite to the cell where the egg was laid, and put down the date.

From these animals forming colonies, and from a vast variety of effects being produced, and with a degree of attention and nicety, that seem even to vie with man; man, not being in the least jealous, has wished to bestow on them more than they possess, viz. a reasoning faculty; while every action is only instinctive, and what they cannot avoid or alter, except from necessity, not from fancy. They have been supposed to be legislators, even mathematicians: indeed, upon a superficial view, there is some shew of reason for such suppositions; but people have gone much further, and have filled up from their imagination every blank, but in so unnatural a way, that one reads it, as if it were the description of a monster. Probably, the best way of treating the history of this insect, is only to describe what is, and the reader will immediately see where authors have been inventing; however, there are some assertions that should be particularly taken notice of, such as forming queen bees at pleasure.

Countries that have but little variety in their seasons may have insects, whose œconomy is well adapted to this uniformity, and which would not be suited to a climate whose seasons are very different; for insects of countries, whose seasons are strongly marked, as in this, have a period in their life which it is little in our power to investigate, and can scarcely be discovered but by accident, for experiments often give little assistance; therefore we are obliged to fill up this blank by reasoning, and from analogy, where we have any. This period is principally the winter, in those insects who live through that season. Animals of season are somewhat like most vegetables; while the common bee is only an animal of seasons in the common actions of life, or what may be called its voluntary actions, and therefore is somewhat like the human species, suited to every country; which may be the reason why it is so universal an animal, for I believe bees are one of the most universal animals known: yet this may arise from cultivation, in consequence of which, they have been brought into climates, where, of themselves, they would not have come.

Insects are so small, and so few of them are capable of being domesticated, that the duration of their life is not easily ascertained; therefore we are to rely more on circumstantial, than on positive or demonstrative proof; and perhaps the life of the common bee may be least in our power to know, for their numbers in the same society make it almost impossible to be ascertained. From their forming a colony, or society, which keeps stationary, the continuance of this society is known, but to what age the individual lives, is not known; we are certain, however, that it is only the labourers and queens that continue the society, for the males die the same year they are formed. From their fixing on the branches of trees, under projecting exposed surfaces, when they swarm, we should be inclined to suppose that they were animals of a warm climate; yet their providing liberally for the change of climate, or rather for a change of season, would, on the contrary, make us believe they were adapted for changeable climates; or rather, these two circumstances should make us suppose they were fitted for both; and their universality proves it. And I do conceive, that in a pretty uniform warm climate, their œconomy may be somewhat different from what it is in the changeable, as they would not be under the same necessity to lay up so much store, and probably might employ their cells in breeding, for a much longer period: however, a good climate agrees with them best, as also a good season in an indifferent climate, such as Britain. We find the common bee in Europe, Asia, Africa, and America. That they may be, or should be in the three first, is easily supposed, but how they came to America is not so readily conceived; for although a kind of manageable animal, yet they do not like such long confinement in their hives, as would carry them to the West Indies, excepting in an ice-house; for when I have endeavoured to confine them in their hives, they have been so restless as to destroy themselves.

The female and the working bee, I believe, in every species have stings, which renders them an animal of offence, indeed, but rather of defence; for although they make an attack, I believe it is by way of defence, excepting when they attack one another, which is seldom or never with their stings. As this belongs more to the labourers, it shall be considered when I treat of them in particular. Of the whole bee tribe, the common bee is the easiest irritated; for as they have property, they are jealous of it, and seem to defend it; but when not near it, they are quiet, and must be hurt before they will sting; with all this disposition for defence, which is only to secure their property, or themselves, when more closely attacked, yet they have no covetousness, nor a disposition to obstruct others. Thus two bees or more will be sucking at the same flower, without the first possessor claiming it as his right: a hundred may be about the same drop of honey, if it is beyond the boundaries of their own right; but what they have collected they defend. It is easily known when they mean to sting; they fly about the object of their anger very quickly, and by the quickness of their motion evade being struck or attacked; which is discovered by the sound of their wings, as if going to give a stroke as they fly, a very different noise from that of the wings when coming home of a fine evening loaded with farina, or honey; it is then a soft contented noise. When a single bee is attacked by several others, it seems the most passive animal possible, making no resistance, and even hardly seeming to wish to get away; and in this manner they allow themselves to be killed. They are perhaps the only insect that feeds in the winter, and therefore the only one that lays up external store; and as all animals, whether insects or not, that keep quiet in the winter, without either eating at all, or eating very little in proportion to what they do in the summer, grow fat and muscular in the summer, (which I term internal store,) we see why the common bee need not be fatter at one time than another; and accordingly we find them nearly of the same fatness the year round.

There are accidents befalling hives of bees, that are not easily accounted for. I had a hive which in the month of November was become quite empty of bees, and upon examination had no honey in it, which was strong in the summer, and had violent attacks made upon it in October by wasps belonging to a nest in the garden, but appeared quiet when that nest was removed. Upon examining this hive, I found only five dead bees, and not a drop of honey in any one cell: there was a good deal of bee bread in different cells scattered up and down the comb, which was become white with mould on its surface. On the other hand, I have had swarms die in the winter in the hives, while there was great plenty of honey in the combs: what seemed remarkable, they all died with their probosces elongated, and in those which I opened, I found the stomachs full of honey, and their intestines full also of excrement, especially the last part.

Of the Heat of Bees.

Bees are, perhaps, the only insect that produces heat within itself, and were therefore intended to have a tolerably well-regulated warmth, without which, of course, they are very uncomfortable, and soon die; and which makes not only a part of their internal œconomy respecting the individual, but a part of their external, or common œconomy, and is therefore necessary to be known. The heat of bees is ascertainable by the thermometer, and I shall give the result of experiments made at two different seasons of the year.

July 18th, at ten in the evening, wind northerly, thermometer at 54°, in the open air, I introduced it into the top of a hive full of bees, and in less than five minutes it rose to 82°. I let it stand all night; at five in the morning it was down at

79°; at nine the same morning, it had risen to 83°, and at one o'clock to 84°; and at nine in the evening it was down to 78°.

December 30th, air at 35°, bees at 73°.

Although bees support a heat nearly equal to that of a quadruped, yet their external covering is not different from that of insects which do not; there is no difference between their coat and a common fly's or wasp's, nor are they fatter, all which makes them bad retainers of heat; therefore they are chilly; and in a cold too severe for them to be comfortable in, they make up for their want of size singly, and get into clusters. A single bee has so little power of keeping itself warm, that it presently becomes numbed, and almost motionless; a common night in summer will produce this effect: a cold capable of producing such effects kills them soon, by which means vast numbers die; therefore a common bee is obliged to feed and live in society, to keep itself warm in cold weather. We know that the consumption of heat may be greater than the power of forming it; when that is the case, we become sensible of it, and then take on such actions as are either instinctive, such as arise naturally out of the impression, or as reason, custom, or habit direct. Many animals, upon the impression of cold, coil themselves up in their own fur, bringing all their extremities into the centre, or hollow of the belly; birds bring their feet under the belly, and thrust their bill between their wing and body; many, if not all, go to the warmest places, either from instinctive principle, or habit: but the bees have no other mode but forming clusters, and the larger the better. As they are easily affected by cold, their instinctive principle respecting cold is very strong, as likewise with regard to wet.

I have seen a swarm hanging out at the door of a hive, ready to take flight, and then return; a chill has come on, of which I was not sensible, and in a few minutes the whole has gone back into the hive; and by the cold increasing, I have at length perceived the cause of their return. If rain is coming on, we observe them returning home in great quantities, and hardly any abroad. The eggs of bees require this heat as much as themselves, nor will the maggot live in a cold of 60° or 70°, nor even their chrysalis. This warmth keeps the wax so soft, as to allow them to model it with ease. In glass hives, or those that have windows of glass in them, we often find a dew on the inside of the glass, especially when the glass is colder than the air within: whether this is perspiration from the bees, both from their external surface and lungs, or evaporation from the honey, I cannot say.

Bees are very cleanly animals respecting themselves, although not so respecting the remains of their young. They, I believe, seldom or never evacuate their excrement in the hive. I have known them confined many days without discharging the contents of the rectum; and the moment they got abroad, they evacuated in the air, when flying: and they appear to be very nice in their bodies, for I have often detected them cleaning one another, more especially if by accident they are besmeared with honey.

This animal may be considered alone, or so far as concerns its own œconomy as an individual, which is common to the most solitary animals; but it can also be considered as a member of society, in which it is taking an active part, and in which it becomes an object of great curiosity.

To consider this society individually, it may be said to consist of a female breeder, female non-breeders, and males: but to consider it as a community, it may be said to consist only of female breeders and non-breeders, the males answering no other purpose than simply as a male, and are only temporary; and it is probable, the female breeder is to be considered in no other light than as a layer of eggs, and that she only influences the non-breeders by her presence, being only a bond of union, for without her they seem to have no tie; it is her presence that makes them an aggregate animal. May we not suppose that the offspring of the queen have an attachment to the mother, somewhat similar to the attachment of young birds to the female that brings them up? for although the times of their attachment are not equal, yet it is the dependence which each has on its mother, that constitutes the bond; for bees have none without her: however, the similarity is not exact, for young animals who have lost their nurse will herd together, and jointly make the best shifts they can, because in future they are to become single animals; but bees have an eternal instinctive dependence on the mother, probably from there not being distinct sexes. When the queen is lost, this attachment is broke; they give up industry, probably die; or, we may suppose, join some other hive. This is not the case with those of this tribe, whose queen singly forms a colony; for although the queen is destroyed, yet they go on with that work which is their lot; as the wasp, hornet, and humble bee. Most probably the whole œconomy of the bee, which we so much admire, belongs to the non-breeders, and depends on their instinctive powers being set to work by the presence of the breeders, that being their only enjoyment; therefore when we talk of the wonderful œconomy of bees, it is chiefly the labourers at large we are to admire, although the queen gets the principal credit, for the extent of *their* instinctive properties.

This œconomy, in its appearances and operations, is somewhat similar to human society, but very different in its first causes and mode of conduct. The human species sets up its own standard; the bee has one set up by nature, and therefore fulfils all the necessary purposes. This standard of influence, which is the breeder, is called the queen, and I shall keep to the name, although I do not allow her voluntary influence or power.

The non-breeders are what compose the hive, or what may be called the community at large; and the males, are mere males: each of these parts of the community I shall hereafter consider separately.

To take up the common bee in any one period of the year, or, in other words, in any one month, and carry it round to the same, and observe what happens in that time, is probably including the whole œconomy of bees; for although they may live more than one year, which I believe is not known, from its not being easily ascertained, yet each year can only be a repetition of the last, as I conceive they are complete in the first; therefore the history of one year may be said to make a whole, and of course it is not material at what time in the circle we begin the history.

Perhaps the best time to begin the history of such insects, as only come to full growth the season they are bred, and live through the winter, and breed the summer following, is when they emerge from the torpid state, and begin to breed; but it might be thought that the common bee is an exception to this rule, because they begin early in the spring to breed, generally before they can be observed; and as they breed to form a colony, which is to go off from the old stock, in order to set out anew, it might seem most natural to begin with this colony, and trace it through its various actions of life for one year, when it, as it were, regenerates itself, and comes round to the same point again, that the old stock was in when it threw off this colony.

Bees, like every other animal that is taken care of in the time of breeding, or incubation, and nursed to the age of taking care of itself, cannot be said to have a period in which we can begin its natural history; but in some other insects there is such a period, for they can be traced from an egg, becoming totally independent of the parent from the moment of being laid, as the silk-worm, &c. There are three periods at which the history of the bee may commence: first, in the spring, when the queen begins to lay her eggs; in the summer, at the commencement of a new colony; or in the autumn, when they are going into winter-quarters. I shall begin the particular history of the bee with the new colony, when nothing is formed; for it begins then every thing that can possibly happen afterwards.

When a hive sends off a colony, it is commonly in the month of June, but that will vary according to the season, for in a mild spring bees sometimes swarm in the middle of May, and very often at the latter end of it. Before they come off, they commonly hang about the mouth of the hole, or door of the hive, for some days, as if they had not sufficient room within for such hot weather, which I believe is very much the case; for if cold or wet weather come on, they stow themselves very well, and wait for fine weather. But swarming appears to be rather an operation arising from necessity, for they would seem not naturally to swarm, because if they have an empty space to fill, they do not swarm; therefore by increasing the size of the hive, the swarming is prevented. This period is much longer in some than in others. For some evenings before they come off, is often heard a singular noise, a kind of ring, or sound of a small trumpet; by comparing it with the notes of the piano-forte, it seemed to be the same sound with the lower A of the treble.

The swarm commonly consists of three classes; a female, or females,* males, and those commonly called mules, which are supposed to be of no sex, and are the labourers; the whole about two quarts in bulk, making about six or seven thousand. It is a question that cannot easily be determined, whether this old stock sends off entirely young of the same season, and whether the whole of their young ones, or only part. As the males are entirely bred in the same season, part go off; but part must stay, and most probably it is so with the others. They commonly come off in the heat of the day, often immediately after a shower; who takes the lead I do not know, but should suppose it was the queen. When one goes off, they all immediately follow, and fly about seemingly in great confusion, although there is one principle actuating the whole. They soon appear to be directed to some fixed place; such as the branch of a tree or bush, the cavities of old trees, holes of houses leading into some hollow place; and whenever the stand is made, they all immediately repair to it, till they are all collected. But it would seem, in some cases, that they had not fixed upon any resting place before they came off, or if they had, that they were either disturbed, if it was near, or that it was at a great distance; for, after hovering some time, as if undetermined, they fly away, mount up into the air, and go off with great velocity. When they have fixed upon their future habitation, they immediately begin to make their combs, for they have the materials within themselves. I have reason to believe that they fill their crops with honey when they come away; probably from the stock in the hive. I killed several of those that came away, and found their crops full, while those that remained in the hive had their crops not near so full: some of them came away with farina on their legs, which I conceive to be rather accidental. I may just observe here, that a hive commonly sends off two, sometimes three swarms in a summer; but that the second is commonly less than the first, and the third less than the second; and this last has seldom time to provide for the winter: they shall often threaten to swarm, but do not; whether the threatening is owing to too many bees, and their not swarming is owing to there being no queen, I do not know. It sometimes happens that the swarm shall go back again; but in such instances I have reason to think that they have lost their queen, for the hives to which their swarm have come back do not swarm the next warm day, but shall hang out for a fortnight, or more, and then swarm; and when they do, the swarm is commonly much larger than before, which makes me suspect that they waited for the queen that was to have gone off with the next swarm.

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* I have reason to believe that never more than one female comes off with a swarm.

So far we have set the colony in motion. The materials of their dwelling, or comb, which is the wax, is the next consideration, with the mode of forming, preparing, or disposing of it. In giving a totally new account of the wax, I shall first show it can hardly be what it has been supposed to be. First, I shall observe that the materials, as they are found composing the comb, are not to be found in the same state (as a composition) in any vegetable, where they have been supposed to be got. The substance brought in on their legs, which is the farina of the flowers of plants, is, in common, I believe, imagined to be the materials of which the wax is made, for it is called by most the wax: but it is the farina, for it is always of the same colour as the farina of the flower where they are gathering; and indeed we see them gathering it, and we also see them covered almost all over with it, like a dust; nevertheless, it has been supposed to be the wax, or that the wax was extracted from it. REAUMUR is of this opinion. I made several experiments to see if there was such a quantity of oil in it, as would account for the quantity of wax to be formed, and to learn if it was composed of oil. I held it near the candle; it burnt, but did not smell like wax, and had the same smell, when burning, as farina when it was burnt. I observed that this substance was of different colours on different bees, but always of the same colour on both legs of the same bee; whereas new made comb was all of one colour. I observed, that it was gathered with more avidity for old hives, where the comb is complete, than for those hives where it is only begun, which we could hardly conceive if it was the materials of wax: also we may observe, that at the very beginning of a hive, the bees seldom bring in any substance on their legs for two or three days, and after that the farina gatherers begin to increase; for now some cells are formed to hold it as a store, and some eggs are laid, which when hatched will require this substance as food, and which will be ready when the weather is wet. I have also observed, that when the weather has either been so cold, or so wet, in June, as to hinder a young swarm from going abroad, they have yet in that time formed as much new comb, as they did in the same time when the weather was such as allowed them to go abroad. I have seen them bring it in about the latter end of March, and have observed, in glass hives, the bees with the farina on their legs, and have seen them disposing of it, as will be described hereafter.

The wax is formed by the bees themselves; it may be called an external secretion of oil, and I have found that it is formed between each scale of the under side of the belly. When I first observed this substance, in my examination of the working bee, I was at a loss to say what it was: I asked myself if it was new scales forming, and whether they cast the old, as the lobster, &c. does? but it was to be found only between the scales, on the lower side of the belly. On examining the bees through glass hives, while they were climbing up the glass, I could see that most of them had this substance, for it looked as if the lower, or posterior edge of the scale, was double, or that there were double scales; but I perceived it was loose, not attached. Finding that the substance brought in on their legs was farina, intended, as appeared from every circumstance, to be the food of the maggot, and not to make wax; and not having yet perceived any thing that could give me the least idea of wax; I conceived these scales might be it, at least I thought it necessary to investigate them. I therefore took several on the point of a needle, and held them to a candle, where they melted, and immediately formed themselves into a round globe; upon which I no longer doubted but this was the wax, which opinion was confirmed to me by not finding those scales but in the building season. In the bottom of the hive we see a good many of the scales lying loose, some pretty perfect, others in pieces. I have endeavoured to catch them, either taking this matter out of themselves, from between the scales of the abdomen, or from one another, but never could satisfy myself in this respect: however, I once caught a bee examining between the scales of the belly of another, but I could not find that it took any thing from between. We very often see some of the bees wagging their belly, as if tickled, running round, and to and fro, for only a little way, followed by one or two other bees, as if examining them. I conceived they were probably shaking out the scales of wax, and that the others were ready upon the watch to catch them, but I could not absolutely determine what they did. It is with these scales that they form the cells called the comb, but perhaps not entirely, for, I believe, they mix farina with it; however, this only occasionally, when probably the secretion is not in great plenty. I have some reason to think, that where no other substance is introduced, the thickness of the scale is the same with that of the sides of the comb; if so, then a comb may be no more than a number of these united; but a great deal of the comb seems to be too thick for this, and, indeed, would appear to be a mixture, similar to the covering of the chrysalis. The wax naturally is white, but when melted from the comb at large, it is yellow. I apprehended this might arise from its being stained with honey, the excrement of the maggots, and with the bee-bread. I steeped some white comb in honey, boiled some with farina, as also with old comb, but I could not say that it was made yellower. Wax, by bleaching, is brought back to its natural colour, which is also a proof that its colour is derived from some mixture. I have reason to believe that they take the old comb, when either broken down, or by any accident rendered useless, and employ it again; but this can only be with combs that have had no bees hatched in them, for the wax cannot be separated from the silk afterwards. Reaumur supposed that they new worked up the old materials, because he found the covering of the chrysalis of a yellower colour than the other parts of the new comb; but this is always so, whether they have old yellow comb to work up, or not, as will be shewn.

The bees who gather the farina, also form the wax, for I found it between their scales.

The cells, or rather the congeries of cells, which compose the comb, may be said to form perpendicular plates, or partitions, which extend from top to bottom of the cavity in which they build them, and from side to side. They always begin at the top, or roof of the vault, in which they build, and work downwards; but if the upper part of this vault, to which their combs are fixed, is removed, and a dome is put over, they begin at the upper edge of the old comb, and work up into the new cavity at the top. They generally may be guided as to the direction of their new plates of comb, by forming ridges at top, to which they begin to attach their comb. In a long hive, if these ridges are longitudinal, their plates of comb will be longitudinal; if placed transverse, so will be the plates; and if oblique, the plates of comb will be oblique. Each plate consists of a double set of cells, whose bottoms form the partition between each set. The plates themselves are not very regularly arranged, not forming a regular plane where they might have done so; but are often adapted to the situation, or shape of the cavity in which they are built. The bees do not endeavour to shape their cavity to their work, as the wasps do, nor are the cells of equal depths, also fitting them to their situation; but as the breeding cells must all be of a given depth, they reserve a sufficient number for breeding in, and they put the honey into the others, as also into the shallow ones. The attachment of the comb round the cavity is not continued, but interrupted, so as to form passages; there are also passages in the middle of the plates, especially if there be a cross stick to support the comb; these allow of bees to go across from plate to plate. The substance which they use for attaching their combs to surrounding parts is not the same as the common wax; it is softer and tougher, a good deal like the substance with which they cover in their chrysalis, or the humble bee surrounds her eggs. It is probably a mixture of wax with farina. The cells are placed nearly horizontally, but not exactly so; the mouth raised a little, which probably may be to retain the honey the better; however this rule is not strictly observed, for often they are horizontal, and towards the lower edge of a plane of comb they are often declining. The first combs that a hive forms are the smallest, and much neater than the last, or lowermost. Their sides, or partitions between cell and cell, are much thinner, and the hexagon is much more perfect. The wax is purer, being probably little else but wax, and it is more brittle. The lower combs are considerably larger, and contain much more wax, or perhaps, more properly, more materials; and the cells are at such distances as to allow them to be of a round figure: the wax is softer, and there is something mixed with it. I have observed that the cells are not all of equal size, some being a degree larger than the others; and that the small are the first formed, and of course at the upper part, where the bees begin, and the larger are nearer the lower part of the comb, or last made: however, in hives of particular construction, where the bees may begin to work at one end, and can work both down, and towards the other end, we often find the larger cells both on the lower part of the combs, and also at the opposite end. These are formed for the males to be bred in; and in the hornets and wasps combs, there are larger cells for the queens to be bred in: these are also formed in the lower tier, and the last formed.

The first comb made in a hive, is all of one colour, viz. almost white; but it is not so white towards the end of the season, having then more of a yellow cast.

Of the Royal Cell.

There is a cell, which is called the Royal Cell, often three or four of them, sometimes more; I have seen eleven, and even thirteen in the same hive; commonly they are placed on the edge of one or more of the combs, but often on the side of a comb; however, not in the centre, along with the other cells, like a large one placed among the others, but often against the mouths of the cells, and projecting out beyond the common surface of the comb; but most of them are formed from the edge of the comb, which terminates in one of these cells. The royal cell is much wider than the others, but seldom so deep: its mouth is round, and appears to be the largest half of an oval in depth, and is declining downwards, instead of being horizontal, or lateral. The materials of which it is composed are softer than common wax, rather like the last mentioned, or those of which the lower edge of the plate of comb is made, or with which the bees cover the chrysalis: they have very little wax in their composition, not one third, the rest I conceive to be farina.

This is supposed to be the cell in which the queen is bred, but I have reason to believe that this is only imagination: for, first, it is too large, and, moreover, seldom so deep as the large cells in which the males are bred; whereas, if proportioned to the length of the queen, it ought to be deeper, for length of body is her greatest difference. In the second place, its mouth is placed downward; and in the third place, it is never lined with the silken covering of the chrysalis, similar to the cells of the males and labourers; nor do we find excrement at the bottom of it. The number of these cells is very different, in different hives. I think I have seen hives without any, and I have seen them with eleven or twelve, sometimes more. I have examined them at all times through the summer, but never found any alteration in them.

The comb seems at first to be formed for propagation, and the reception of honey to be only a secondary use; for if the bees lose their queen, they make no combs; and the wasp, hornet, &c. make combs, although they collect no honey; and the humble bee collects honey, and deposits it in cells she never made.

I shall not consider the bee as an excellent mathematician, capable of making exact forms, and having reasoned upon the best shape of the cell for capacity, so that the greatest number might be put into the smallest space (for the hornet and the wasp are much more correct, although not seemingly under the same necessity, as they collect nothing to occupy their cells); because, although the bee is pretty perfect in these respects, yet it is very incorrect in others, in the formation of the comb: nor shall I consider these animals as forming comb of certain shape and size, from mere mechanical necessity, as from working round themselves; for such a mould would not form cells of different sizes, much less could wasps be guided by the same principle, as their cells are of very different sizes, and the first by much too small for the queen wasp to have worked round herself: but I shall consider the whole as an instinctive principle, in which the animal has no power of variation, or choice, but such as arises from what may be called external necessity. The cell has in common six sides, but this is most correct in those first formed; and their bottom is commonly composed of those sides, or planes, two of the sides making one; and they generally fall in between the bottoms of three cells of the opposite side; but this is not regular, it is only to be found where there is no external interruption.

I have already observed, that the last formed cells in the season are not so well made: that their partitions are thicker, and more of a yellow colour: this arises, I imagine, from the wax being less pure, having more alloy in it; and therefore, not being so strong, more of it is required. The bees would appear to reserve many of their cells for honey, and those are mostly at the upper part. In old hives, of several years standing, I have found the upper part of the comb free from the consequences of having bred, such as the silk lining, and the excrement of the maggots at the bottom; while the lower part, for probably more than one half of the plane of cells, shewed strong marks of having contained many broods of young bees.

In such the lining of silk is thick at the sides, composed of many laminæ; and in many, the bottom is half filled up with excrement; and I observed at such parts, the comb was thickest at its mouth, which inclines me to think, that when a cell becomes shallow, by the bottom being in some degree filled up, the bees then add to its mouth. Such also they seem to reserve principally for the bee-bread; so that to lay up a greater store of honey is an object to them.

Of the Laying of Eggs.

As soon as a few combs are formed, the female bee begins laying of eggs. As far as I have been able to observe, the queen is the only bee that propagates, although it is asserted that the labourers do. Her first eggs in the season are those which produce labourers; then the males, and probably the queen; this is the progress in the wasp, hornet, humble bee, &c. However, it is asserted by RIEM, that when a hive is deprived of a queen, labourers lay eggs; also, that at this time, some honey and farina are brought in, as store for a wet day. The eggs are laid at the bottom of the cell, and we find them there before the cells are half completed, so that propagation begins early and goes on along with the formation of the other cells. The egg is attached at one end to the bottom of the cell, sometimes standing perpendicularly, often obliquely; it has a glutinous, or slimy covering, which makes it stick to any thing it touches. It would appear that there was a period or periods for laying eggs; for I have observed in a new swarm, that the great business of laying eggs did not last above a fortnight; although the hive was not half filled with comb, it began to slacken.

Probably that end of the egg which is first protruded, is that which sticks to the bottom of the cell: and probably the tail of the maggot is formed at that end: when they move the egg, how they make it stick again, I do not know. I have just observed, that they often move the egg out of a cell, to some other, we may suppose; why they do this, I cannot say; whether it is because we have been exposing this part, is not easily determined. In those new formed combs, as also in many not half finished, we find the substance called bee-bread, and some of it is covered over with wax; which will be considered further. By the time they have worked above half way down the hive, with the comb, they are beginning to form the larger cells, and by this time the first broods are hatched, which were small, or labourers; and now they begin to breed males, and probably a queen, for a new swarm; because the males are now bred to impregnatethe young queen for the present summer, as also for the next year. This progress in breeding is the same with that of the wasp, hornet, and humble bee.* Although *this account* is commonly allowed, yet writers on this subject have supposed another mode of producing a queen, when the hive is in possession of maggots, and deprived of their queen.

What may be called the complete process of the egg, namely, from the time of laying to the birth of the bee, (that is, the time of hatching,) the life of the maggot, and the life of the chrysalis, is, I believe, shorter than in most insects. It is not easy to fix the time when the eggs hatch: I have been led to imagine it was in five days. When they hatch, we find the young maggot lying coiled up in the bottom of the cell, in some degree surrounded with a transparent fluid. In many of the cells, where the eggs have just hatched, we find the skin standing in its place, either not yet removed, or not pressed down by the maggot. There is now an additional employment for the labourers, namely, the feeding and nursing the young maggots. We may suppose the queen has nothing to do with this, as there are at all times labourers enough in the hive for such purposes, especially too, as she never does bring the materials, as every other of the tribe is obliged to do at first; therefore she seems to be a queen by hereditary, or rather, by natural right, while the humble bee, wasp, hornet, &c. seem rather to work themselves into royalty, or mistresses of the community. The bees are readily detected feeding the young maggot; and indeed a young maggot might easily be brought up, by any person who would be attentive to feed it. They open their two lateral pincers to receive the food, and swallow it. As they grow, they cast their coats, or cuticles; but how often they throw their coats, while in the maggot state, I do not know. I observed that they often removed their eggs; I also find they very often shift the maggot into another cell, even when very large. The maggots grow larger and larger till they nearly fill the cell; and by this time they require no more food, and are ready to be inclosed for the chrysalis state: how this period is discovered I do not know, for in every other insect, as far as I am acquainted, it is an operation of the maggot, or caterpillar itself; but in the common bee, it is an operation of the perfect animal; probably it arises from the maggot refusing food.

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* REAUMUR on Bees, says, that the drone eggs, when laid in small cells, produce drones: and WILHELMI says, that it is the labourers only that lay drone eggs. Mr. RIEM says, that queens are never reared in any but royal cells, although males sometimes in common cells; and workers in old queen cells, but never in those recently made.

The time between their being hatched and their being inclosed is, I believe, four days; at least, from repeated observations, it comes nearly to that time: when ready for the chrysalis state, the bees cover over the mouth of the cell, with a substance of a light brown colour, much in the same manner that they cover the honey, excepting that, in the present instance, the covering is convex externally, and appears not to be entirely wax, but a mixture of wax and farina. The maggot is now perfectly inclosed, and it begins to line the cell and covering of the mouth above mentioned, with a silk it spins out similar to the silk-worm, and which makes a kind of pod for the chrysalis. BONNET observed, that, in one instance, the cell was too short for the chrysalis, and it broke its covering, and formed its pod higher, or more convex than common: this I can conceive possible; we often see it in the wasp. Having completed this lining, they cast off, or rather shove off, from the head backwards, the last maggot coat, which is deposited at the bottom of the cell, and then they become chrysalises.

Of the Food of the Maggot, or what is commonly called Bee-Bread.

One would naturally suppose, that the food of the maggot bee should be honey, both because it is the food of the old ones, and it is what they appear principally to collect for themselves; however, the circumstance of honey being food for the old ones is no argument, because very few young animals live on the same food with the old, and therefore it is probable the maggot bee does not live upon honey; and if we reason from analogy, we shall be led to suppose the bee-bread to be the food of the maggot. It is the food of the maggot of the humble bee, who feeds upon honey, and even lays up a store of honey for a wet day, yet does not feed the young with it. It is the food of the maggot of a black bee, and also of several others of the solitary kind, who also feed upon honey; and wasps, &c. who do not bring in such materials, do not feed themselves upon honey. We cannot suppose, that the bee-bread is for the food of the old bees, when we see them collecting it in the months of June, July, &c. at which time they have honey in great plenty. This substance is as common to a hive as any part belonging to the œconomy of bees. Before they have formed five or six square inches of comb in a young hive, we shall find eggs, honey, and bee-bread; and at whatever time of the year we kill a hive, we shall find this substance; and if a hive is short of honey, and dies in the winter, we find no honey, but all the bee-bread, which was laid up in store for the maggots in the spring. They take great care of it, for it is often covered over with wax, as the honey, and I believe more especially in the winter; probably with a view to preserve it till wanted. In April I have found some of the cells full, others only half full. If we slit down a cell filled with this substance, we shall commonly find it composed of layers of different colours; some a deep orange, others a pale brown. In glass hives, we often find that the glass makes one side of the cell, and frequently in such we shall see at once the different strata above mentioned. This is the substance which they bring in on their legs, and consists of the farina of plants. It is not the farina of every plant that the bee collects, at least they are found gathering it from some with great industry, while we never find them on others: St. John's wort is a favourite plant, but that comes late. The flower of the gourd, cucumber, &c. they seem to be fond of. What they do collect must be the very loose stuff, just ready to be blown off to impregnate the female part of the flower; and to shew that this is the case, we find bees impregnate flowers that have not the male part. It is in common of a yellow colour, but that of very different shades, often of an orange; and when we see bees collecting it on bushes that have a great many flowers, so as to furnish a complete load, it is then of the colour of the farina of that bush. It is curious to see them deposite this substance in the cell. On viewing the hives, we often see bees with this substance on their legs, moving along on the combs, as if looking out for the cell to deposite it in. They will often walk over a cell that has some deposited in it, but shall leave that, and try another, and so on till they fix; which made me conceive that each bee had its own cell. When they come to the intended cell, they put their two hind legs into it, with the two fore legs and the trunk out on the mouth of the neighbouring cell, and then the tail, or belly, is thrust down into the intended cell; they then bring the leg under the belly, and turning the point of the tail to the outside of the leg, where the farina is, they shove it off by the point of the tail. When it is thus shoved off both legs, the bee leaves it, and the two pieces of farina may be seen lying at the bottom of the cell: another bee comes almost immediately, and creeping into the cell, continues about five minutes, kneading and working it down into the bottom, or spreads it over what was deposited there before, leaving it a smooth surface.

It is of a consistency like paste; burns slightly, and gives a kind of unusual smell, probably from having been mixed with animal juice in the act of kneading it down; for when brought in, it is rather a powder than a paste. That it is the food of the maggot is proved by examining the animal's stomach; for when we kill a maggot full grown, we find its stomach full of a similar substance, only softer, as if mixed with a fluid, but we never find honey in the stomach; therefore we are to suppose it is collected as food for the maggot, as much as honey is for the old bee. Mr. SCHIRACH imagines, that the semen of the male is the food of the maggot; but the food of the male and the queen maggot has been supposed to be different from that of the labourers. REAUMUR says, the food of the queen maggot is different in taste from that of the common ones. How he knew this, who was unacquainted with the food of the others, I cannot conceive.

Of the Excrement of the Maggot.

They have very little excrement, but what they do discharge is deposited at the bottom of the cell; and what at first will appear rather extraordinary, it is never cleared away by the bees, but allowed to dry along with the maggot coats; and both fresh eggs and honey are deposited in these cells, so circumstanced, every future year; so that in time the cells become nearly half full.

Of the Chrysalis State.

In this state they are forming themselves for a new life: they are either entirely new built, or wonderfully changed, for there is not the smallest vestige of the old form remaining;

yet it must be the same materials, for now nothing is taken in. How far this change is only the old parts new modelled, or gradually altering their form, is not easily determined. To bring about the change, many parts must be removed, out of which the new ones are probably formed. As bees are not different in this state from the common flying insects in general, I shall not pursue the subject of their changes further; although it makes a very material part in the natural history of insects.

When the chrysalis is formed into the complete bee, it then destroys the covering of its cell, and comes forth. The time it continues in this state is easier ascertained than either in that of the egg, or the maggot; for the bees cannot move the chrysalis, as they do the two others. In one instance it was thirteen days and twelve hours exactly; so that an egg in hatching being five days, the age of the maggot being four days, and the chrysalis continuing thirteen and a half, the whole makes twenty-two days and a half: but how far this is accurate, I will not pretend to say. I found that the chrysalis of a male was fourteen days, but this was probably accidental. When they first come out, they are of a greyish colour, but soon turn brown.

When the swarm of which I have hitherto been giving the history has come off early, and is a large one, more especially if it was put into too small a hive, it often breeds too many for the hive to keep through the winter; and in such case a new swarm is thrown off, which, however, is commonly not a large one, and generally has too little time to complete its comb, and store it with honey sufficient to preserve them through the winter. This is similar to the second or third swarm of the old hives.

Of the Seasons, when the different Operations of Bees take place.

I have already observed, that the new colony immediately sets about the increase of their numbers, and every thing relating to it. They had their apartments to build, both for the purpose of breeding, and as a storehouse for provisions for the winter. When the season for laying eggs is over, then is the season for collecting honey; therefore, when the last chrysalis for the season comes forth, its cell is immediately filled with honey, and as soon as a cell is full, it is covered over with pure wax, and is to be considered as store for the winter. This covering answers two very essential purposes: one is to keep it from spilling, or daubing the bees: the other to prevent its evaporation, by which means it is kept fluid in such a warmth. They are also employed in laying up a store of bee-bread for the young maggots in the spring, for they begin to bring forth much earlier than probably any other insect, because they retain a summer heat, and store up food for the young.

In the month of August we may suppose the queen, or queens, are impregnated by the males; and as the males do not provide for themselves, they become burdensome to the workers, and are therefore teased to death much sooner than they otherwise would die; and when the bees set about this business, of providing their winter store, every operation is over, except the collecting of honey and bee-bread. At this time it would seem as if the males were conscious of their danger, for they do not rest on the mouth of the hive in either going out or coming in, but hurry either in, or out: however they are commonly attacked by one, two, or three at a time: they seem to make no resistance, only getting away as fast as possible. The labourers do not sting them, only pinch them, and pull them about as if to wear them out; but I suspect it may be called as much a natural, as a violent death.

The whole of the males are now destroyed, and indeed it would have been useless to have saved any to impregnate the queen in the spring. That there may be many more than may be wanted, I can easily believe, for this we see throughout nature; but she always times her operations well, although there may be supernumeraries.

When the young are wholly come forth, and either the cells entirely filled, or no more honey to be collected, then is the time, or season, for remaining in their hives for the winter.

Although I have now completed a hive, and no operations are going on in the winter months, yet the history of this hive is imperfect till it sends forth a new swarm.

As the common bee is very susceptible of cold, we find as soon as the cold weather sets in, they become very quiet, or still, and remain so throughout the winter, living on the produce of the summer and autumn; and indeed a cold day in the summer is sufficient to keep them at home, more so than a shower in a warm day: and if the hive is thin, and much exposed, they will hardly move in it, but get as close together as the comb will let them, into a cluster. In this manner they appear to live through the winter: however, in a fine day, they become very lively and active, going abroad, and appearing to enjoy it, at which time they get rid of their excrement; for I fancy they seldom throw out their excrement when in the hive. To prove this, I confined some bees in a small hive, and fed them with honey for some days; and the moment I let them out, they flew, and threw out their excrement in large quantities; and therefore, in the winter, I presume, they retain the contents of their bowels for a considerable time: indeed, when we consider their confinement in the winter, and that they have no place to deposite their excrement, we can hardly account for the whole of this operation in them. Their excrement is of a yellow colour, and according to their confinement it is found higher and higher up in the intestine, almost as high as the crop.

Their life at this season of the year is more uniform, and may be termed simple existence, till the warm weather arrives again. As they now subsist on their summer's industry, they would seem to feed in proportion to the coldness of the season; for from experiment, I found the hive grow lighter in a cold week, than it did in a warmer, which led to further experiments. I first made an experiment upon a bee hive, to ascertain the quantity of honey lost through the winter. The hive was put into the scale November the 3d, 1776.

		oz.	drams.
November 10th	it had lost	2	7
17th	——	4	2½
24th	——	3	7½
December 1st	——	8	2
8th	——	2	1
15th	——	5	2
22d	——	4	3
29th	——	5	4
1777. January 1st	——	2	5

		oz.	drams.
January 12th	it had lost	5	2
19th	——	3	4
26th	——	3	1½
February 2d	——	5	0
9th	——	7	0
	The whole	72	1½

Although an indolent state is very much the condition of bees through the winter, yet progress is making in the queen towards a summer's increase. The eggs in the oviducts are beginning to swell, and, I believe, in the month of March she is ready to lay them, for the young bees are to swarm in June; which constitutes the queen bee to be the earliest breeder of any insect we know. In consequence of this, the labourers become sooner employed than any other of this tribe of insects. This both queen and labourers are enabled to accomplish, from living in society through the winter; and it becomes necessary in them, as they have their colony to form early in the summer, which is to provide for itself for the winter following. All this requires the process to be carried forward earlier than by any other insect, for these are only to have young which are to take care of themselves through the summer, not being under the necessity of providing for the winter.

In the month of April, I found in the cells, young bees, in all stages, from the egg to the chrysalis state; some of which were changed in their colour, therefore, were nearly arrived at the fly state, and probably some might have flown.

As this season is too early for collecting the provision of the maggot abroad, the store of farina comes now into use; but as soon as flowers begin to blow, the bees gather the fresh, although they have farina in store, giving the fresh the preference.

Of the Queen.

The queen bee, as she is termed, has excited more curiosity than all the others, although much more belongs to the labourers. From the number of these, and from their exposing themselves, they have their history much better made out: but as there is only one queen, and she scarcely ever seen, it being only the effects of her labour we can come at, an opportunity has been given to the ingenuity of conjecture, and more has been said than can well be proved. She is allowed to be bred in the common way, only that there is a peculiar cell for her in her first stage; and REAUMUR says, " her food is different when in the maggot state;" but as there is probably but one queen, that the whole might not depend on one life, it is asserted that the labourers have a power of forming a common maggot into a queen. If authors had given us this as an opinion only, we might have passed it over as improbable, but they have endeavoured to prove it by experiments, which require to be examined: and for that purpose, I shall give what they say on that head, with my remarks upon it.

Abstracts from Mr. SCHIRACH.

The following experiments were made to ascertain the origin of the queen bee:—" In twelve wooden boxes were placed " twelve pieces of comb, four inches square, each containing "both eggs and maggots, so suspended that the bees could " come round every part of the comb: in each box was shut " up a handful of working bees. Knowing that when bees " are forming a queen, they should be confined,* the boxes " were kept shut for two days. When examined at the end " of that period (six boxes only were opened), in all of them " royal cells were begun, one, two, or three, in each; all of " these containing a maggot four days old. In four days, the " other six boxes were opened, and royal cells found in each, " containing maggots five days old, surrounded by a large " provision of jelly; and one of these maggots, examined in " the microscope, in every respect resembled a working bee.

" This experiment was repeated, and the maggots se- " lected to be made queens were three days old; and in " seventeen days there were found in the twelve boxes fif- " teen lively, handsome queens.† These experiments were " made in May, and the bees were allowed to work great " part of the summer: the bees were examined one by one, " but no drone could be discovered, and yet the queens were " impregnated, and laid their eggs.‡

" The above experiment was repeated with pieces of comb,

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* How he came to know this, I cannot conceive, for nothing *a priori* could give such information.

† Now this account is not only improbable, but it does not tally with itself. First, it is not probable that a handful of bees should, or would, set about making two, three, or four queens, when we do not find that number in a large hive: and secondly, it seems inconsistent that only fifteen should be formed out of twelve parcels, when some of the former parcels had four young queens.

‡ Here is a wonder of another kind: queens laying eggs, which (we must suppose Mr. Schirach meant we should believe) they hatched, without the influence of the male.

" containing eggs only, in six boxes, but no preparations " were made towards producing a queen.*

" The experiment of producing a queen bee from a maggot " was repeated every month of the year, even in November.†

" A maggot three days old was procured from a friend, in" closed in an ordinary cell, and shut up with a piece of comb, " containing eggs and maggots. That three days old was " formed into a queen, and all the other maggots and eggs " were destroyed.‡

" In above a hundred experiments a queen has been formed " from maggots three days old." §

WILHELMI observes, that a queen cell, which is made while the bees are shut up, is formed by breaking down three common cells into one, when the maggot is placed in the centre, after which the sides are repaired.

A young queen lately hatched was put into a hive, which had been previously ascertained to contain no drones, and whose queen was removed; and yet the young queen laid eggs.|| In repeating Mr. SCHIRACH's experiment, he shut up four

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* Why eggs, which we must conceive hatched, and produced maggots, did not form queens, one cannot imagine.

† In which month, as bees never swarm, there could be no occasion for mothers, or supernumerary queens, and still each experiment produced a handsome queen. This is as singular an observation as any. In this country, and in all similar ones, bees hardly breed after July, and by the beginning of September there is hardly a chrysalis to be seen; yet these bred till November, and even laid eggs.

‡ Why did the bees destroy them in this experiment, and not in others?

§ The working bees, from the above experiments, are considered as all females, although the ovaria are too small for examination.

It would appear that a maggot three days old was of the best age for this experiment, yet one should have conceived that a maggot two days old would soon be fit.

|| There is no mystery in this; but did they hatch?

pieces of comb, with one maggot in each; after two days the maggots were all dead, and the bees had desisted from labour.*

A piece of comb, from which all the eggs and maggots had been removed, was shut up with some honey, and a certain number of workers; in a short time they became very busy, and upon the evening of the second day 300 eggs were found in the cells.† He repeated this experiment with the same result, and the bees were left to themselves: they placed queen maggots in the queen cells, newly constructed, and others in male cells: the rest were left undisturbed. He again took two pieces of comb, which contained neither eggs nor maggots, and shut them up with a certain number of workers, and carried the box into a stove: next evening, one of the pieces of comb contained several eggs, and the beginning of a royal cell, that was empty.

Besides the short observations contained in the notes, I beg leave to observe, that I have my doubts respecting the whole of these experiments, from several circumstances which occurred in mine. The three following facts appear much against their probability: first, a summer's evening in this country is commonly too cold for so small a parcel of bees to be lively, so as to set about new operations; they get so benumbed, that they hardly recover in the day, and I should suspect that where these experiments were made (and indeed some are said to have been tried in this country), it is also too cold: secondly, if the weather should happen to be so warm as to prevent this effect, then they are so restless, that they commonly destroy themselves, or wear themselves out; at least, after a few days confinement we find them mostly dead: and thirdly, the account given of the formation of a royal cell, without mentioning the above inconvenience, which is natural to the experiment, makes me suspect the whole to be fabricated. To obviate the first objection, which I found from experiment to prevent any success that otherwise might arise, I put my parcel of bees, with their comb, in which were eggs, as also maggots, and in some of the trials there were chrysalises,* into a warmer place, such as a glass frame, over tan, the surface of which was covered with mould, to prevent the rising of unwholesome air: but from knowing that the maggot was fed with bee-bread, or farina, I took care to introduce a cell or two with this substance, as also the flowers of plants that produce a great deal of it, likewise some honey for the old ones. In this state my bees were preserved from the cold, as also provided with necessaries; but after being confined several days, upon opening the door of the hive, what were alive came to the door, walked and flew about, but gradually left it, and on examining the combs, &c. I found the maggots dead, and nothing like any operation going on.

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* This is the most probable event in the whole experiments.

† This would show that labourers can be changed into queens at will, and that neither they nor their eggs require to be impregnated; if this was the case, there would be no occasion for all the push in making a queen or a male.

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* I chose to have some chrysalises, for I supposed that if my bees died, or flew away, the chrysalises when they came out, which would happen in a few days, not knowing where to go, might stay and take care of the maggots that might be hatched from the eggs; but, to my surprise, I found that neither the eggs hatched, nor did the chrysalises come forth; all died: from which I began to suspect that the presence of the bees was necessary for bot .

The queen, the mother of all, in whatever way produced, is a true female, and different from both the labourers and the male. She is not so large in the trunk as the male, and appears to be rather larger in every part than the labourers. The scales on the under surface of the belly of the labourers are not uniformly of the same colour, over the whole scale; that part being lighter which is overlapped by the terminating scale above, and the uncovered part being darker: this light part does not terminate in a straight line, but in two curves, making a peak; all which gives the belly a lighter colour in the labouring bees: more especially when it is pulled out or elongated.

The tongue of the female is considerably shorter than that of the labouring bee, more like that of the male: however, the tongues of the labourers are not in all of an equal length, but none have it so short as the queen.

The size of the belly of the female of such animals varies a little, according to the condition they are in: but the belly of the male and the labourer has but little occasion to change its size, as they are at all times nearly in the same condition with regard to fat, having always plenty of provision: but the true female varies very considerably; she is of a different size and shape in the summer to what she is in the winter; and in the winter she has what may be called her natural size and shape: she is, upon the whole, rather thicker than the labourer; and this thickness is also in the belly, which probably arises from the circumstance of the oviduct being in the winter pretty large, and the reservoir for semen full. The termination of the belly is rather more peaked than in the labourers, the last scale being rather narrower from side to side, and coming more to a point at the anus. The scales at this season are more overlapped, which can only be known by drawing them out. In the spring and summer she is more easily distinguished: the belly is not only thicker, but considerably longer than formerly, which arises from the increase of the eggs. We distinguish a queen from the working bee, simply by size, and in some degree by colour; but this last is not so easily ascertained, because the difference in the colour is not so remarkable in the back, and the only view we can commonly get of her is on this part; but when a hive is killed, the best way is to collect all the bees, and spread them on white paper, or put them into water, in a broad, flat-bottomed, shallow, white dish, in which they swim; and by looking at them singly, she may be discovered. As the queen breeds the first year she is produced, and the oviducts never entirely subside, an old queen is probably thicker than a new bred one, unless indeed the oviducts, and the eggs, form in the chrysalis state, as in the silk-worm, which I should suppose they did. The queen is perhaps at the smallest size just as she has done breeding, for as she is to lay eggs by the month of March, she must begin early to fill again; but I believe her oviducts are never emptied, having at all times eggs in them, although but small. She has fat in her belly, similar to the other bees.

It is most probable that the queen which goes off with the swarm is a young one, for the males go off with the swarm to impregnate her, as she must be impregnated the same year, because she breeds the same year.

The queen has a sting similar to the working bee.

Of the Number of Queens in a Hive.

I believe a hive, or swarm, has but one queen, at least I have never found more than one in a swarm, or in an old hive in the winter; and probably this is what constitutes a hive; for when there are two queens, it is likely that a division may begin to take place. Supernumerary queens are mentioned by RIEM, who asserts he has seen them killed by the labourers, as well as the males.

November 18th, 1788, I killed a hive that had not swarmed the summer before, and which was to appearance ready to swarm every day; but when I supposed the season for swarming was over, and it had not swarmed, I began to suspect that the reason why it did not was owing to there being no young queen or queens; and I found only one. This is a kind of presumptive proof that I was right in my conjecture; unless it be supposed, that when they were determined not to swarm, they destroyed every queen except one. In a hive that died, I found no males, and only one queen. This circumstance, that so few queens are bred, must arise from the natural security the queen is in from the mode of their society; for, although there is but one queen in a wasp's, hornet's, and humble bee's nest or hive, yet these breed a great number of queens; the wasp and hornet some hundreds; but not living in society during the winter, they are subject to great destruction, so that probably not one in a hundred lives to breed in the summer. I have said that the queen leaves off laying in the month of July; and now she is to be impregnated by the males before they die. Mr. RIEM asserts, he has seen the copulation between the male and the female, but does not say at what season. I should doubt this; but Mr. SCHIRACH supposes the queen impregnated without copulation. I know not whether he means by this that she is not impregnated at all, and supposes, like Mr. DEBRAW, that the eggs are impregnated after they are laid, by a set of small drones, who pass over the cells, and thrust their tails down into the cell, so as to besmear the egg.* Mr. BONNET does not consider it necessary that the drones should be small for this purpose, for he saw a large drone passing over the cells of a piece of comb, stopping at every one which contained an egg, but at no other, and giving a knock with his tail on the mouth of the cell three times; this he supposed was the mode of impregnating the eggs. The number three has always been a famous number; but it will not do where there are no males, which is the case of a hive in the spring, the time when the queen is most employed in laying eggs; which made him suppose the use of the males was to feed the maggots with their semen. It is probable that the copulation is like that of most other insects. The copulation of the humble bee I have seen: it is similar to the common fly. The sting is extended at the time, and turned up on the back, between the two animals: they are some time in this act. In the hornet it is the same. The circumstances relative to the impregnating the queen not being known, great room has been given for conjecture, which, if authors had presented as conjectures only, it would have shewn their candour; but they have given, what in them were probably conceits, as facts.

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* Mr. DEBRAW, knowing the drones died in the latter end of summer, or the autumn, was obliged to suppose a small set of males, that lived through the winter, for that purpose.

Of the Male Bee.

The male bee is considerably larger than the labourers: he is even larger than the queen, although not so long when she is in her full state with eggs: he is considerably thicker than either, but not longer in the same proportion: he does not terminate at the anus in so sharp a point; and the opening between the two last scales of the back and belly is larger, and more under the belly, than in the female. His proboscis is much shorter than that of the labouring bee, which makes me suspect he does not collect his own honey, but takes that which is brought home by the others; especially as we never find the males abroad on flowers, &c. only flying about the hives in hot weather, as if taking an airing; and when we find that the male of the humble bee, which collects its own food, has as long a proboscis, or tongue, as the female, I think it is from all these facts reasonable to suppose, the male of the common bee feeds at home. He has no sting.

The males, I believe, are later in being bred than the labouring bee. As they are only produced to go off with a hive, they are not so early brought forth; for in the month of April I killed a hive, in which I found maggots and chrysalises, but did not find any males among the latter: the maggots are too young for such investigation; but about the 20th of May we observed males: they are all very much of the same size. In the month of August, probably about the latter end, we may suppose they impregnate the queen for the next year, and about the latter end of the same month, and beginning of September, they are dying, but seem to be hastened to their end by the labourers. In 1791, as early as the 19th of June, I saw the labourers killing the males of a hive, or rather of a swarm, that had not yet swarmed, but was hanging out; this, however, was out of the common course. They appear to be sensible of their fate, for they hurry in and out of the hive as quick as possible, seemingly with a view to avoid the labourers; and we find them attacked by the labourers, who pinch them with their forceps, and when they are so hurt, and fatigued with attempts to make their escape, as not to be able to fly, they are thrown over on the ground, and left to die. That this is the fate of every male bee is easily ascertained, by examining every bee in the hive when killed for the honey, which is after this season; no male being then found in it. BONNET supposes them starved to death, as he never saw wounds on them. In the course of a winter I have killed several hives, some as late as April, and in such a way as to preserve every bee, and after examining every one entirely, I never perceived one male of any kind; although it has been asserted there are two sizes of males, and that the small are preserved through the winter to impregnate the queen.

Of the Labouring Bee.

This class, for we cannot call it either sex, or species, is the largest in number of the whole community: there are thousands of them to one queen, and probably some hundreds to each male, as we shall see by and by. It is to be supposed they are the only bees which construct the whole hive, and that the queen has no other business but to lay the eggs: they are the only bees that bring in materials; the only ones we observe busy abroad; and, indeed, the idea of any other is ridiculous, when we consider the disproportion in numbers, as well as the employment of the others, while the working bee has nothing to take off its attention to the business of the family. They are smaller than either the queen or the males: not all of equal size, although the difference is not very great.

The queen and the working bees are so much alike, that the latter would seem to be females on a different scale: however, this difference is not so observable in the beginning of winter as in the spring, when the queen is full of eggs. They are all females in construction, having the female parts, which are extremely small, and would be easily overlooked by a person not very well acquainted with the parts in the queen: this has been observed by Mr. RIEM; indeed, one might suppose that they were only young queens, and that they became queens after a certain age; but this is not the case. They all have stings, which is another thing that makes them similar to the queen. From their being furnished with an instrument of defence and offence, they are endowed with such powers of mind as to use it, their minds being extremely irritable; so much so, that they make an attack when not meddled with, simply upon suspicion, and when they do attack, they always sting; and yet, from the circumstance of their not being able to disengage the sting, one should suppose they would be more cautious in striking with it. When they attack one another, they seldom use it, only their pincers: yet I saw two bees engaged, and one stung the other in the mouth, or thereabouts, and the sting was drawn from the body to which it belonged, and the one who was stung ran very quickly about with it; but I could not catch that bee, to observe how the sting was situated.

As they are the collectors of honey, much more than what is for their own use, either immediately, or in future, their tongue is proportionably fitted for that purpose: it is considerably longer than that of either the queen or the male, which fits them to take up the honey from the hollow parts of flowers, of considerable depth. The mechanism is very curious, as will be explained further on.

The number of labourers in a hive varies very considerably.

In one hive that I killed, there were - - 3338

In another - - - - 4472

In one that died, there were - - 2432

That I might guess at the number of bees from a given bulk, I counted what number an alehouse pint held, when wet, and found it contained - - 2160

Therefore, as some swarms will fill two quarts, such must consist of near - - - - 9000

Of the Parts concerned in the Nourishment of the Bee.

Animals who only swallow food for themselves, or whose alimentary organs are fitted wholely for their own nourishment, have them adapted to that use only; but in many, these organs are common for more purposes, as in the pigeon, and likewise in the bee. In this last, some of the parts are used as a temporary reservoir, holding both that which is for the immediate nourishment of the animal, and also that which is to be preserved for a future day, in the cells formerly described; this last portion is therefore thrown up again, or regurgitated. As it is the labourers alone in the common bee that are so employed, we might conceive this reservoir would belong only to them; but both the queen and males, both in the common and humble bee, have it, as also, I believe, every one of the bee tribe.

As the bee is a remarkable instance of regurgitation, it is necessary the structure of the parts concerned in this operation, and which are also connected with digestion, should be well considered. Ruminating animals may be reckoned regurgitating animals, but in them it is for the purpose of digestion entirely in themselves. But many birds may be called regurgitating animals, and in them it is for the purpose of feeding their young. Crows fill their fauces, making a kind of craw, out of which they throw back the food when they feed their young: but the most remarkable is the dove tribe, who first fill their craw, and then throw it up into the beak of their young*. The bee has this power to a remarkable degree, not, however, for the purpose of feeding the young, but it is the mode of depositing their store, when brought home.

In none of the above-mentioned regurgitating animals are the reservoirs containing the food, the immediate organ of digestion; nor does the reservoir for the honey in the bee appear to be its stomach.

The tongue of the bee is the first of the alimentary organs to be considered: it is of a peculiar structure, and is probably the largest tongue of any animal we know, for its size. It may be said to consist of three parts respecting its length, having three articulations. One, its articulation with the head, which is in some measure similar to our larynx. Then comes the body of the tongue, which is composed of two parts; one, a kind of base, on which the other, or true tongue, is articulated. This first part is principally a horny substance, in which there is a groove, and it is articulated with the first, or larynx; on the end of this is fixed the true tongue, with its different parts. These two parts of the tongue are as it were inclosed laterally, by two horny scales, one on each side, which are concave on that side next to the tongue; one edge is thicker than the other, and they do not extend so far as the other parts. Each of these scales is composed of two parts, or scales, respecting its length, one articulated with the other: the first of those scales is articulated with the common base, or larynx, at the articulation of the first part of the tongue, and incloses laterally the second part of the tongue, coming as far forwards as the third articulation: on the end of this is articulated the second scale, which continues the hollow groove that incloses the tongue laterally; this terminates in a point. These scales have some hairs on their edge.

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* *Vide* Observations on certain parts of the animal œconomy, p. 191.

On the termination of the second part, is placed the true tongue, having two lateral portions or processes, on each side, one within the other: the external is the largest, and is somewhat similar to the before-mentioned scales. This is composed of four parts, or rather of one large part, on which three smaller are articulated, having motion on themselves. The first, on which the others stand, is articulated at the edges of the tongue, on the basis, or termination of the last described part of the tongue: this has hairs on its edge.

A little further forwards on the edges of the tongue are two small thin processes, so small as hardly to be seen with the naked eye. The middle part of all, of which these lateral parts are only appendages, is the true tongue. It is something longer than any of the before-mentioned lateral portions; and is not horny, as the other parts are, but what may be called fleshy, being soft and pliable. It is composed of short sections, which probably are so many short muscles, as in fish; for they are capable of moving it in all directions. The tongue itself is extremely villous, having some very long villi at the point, which act, I conceive, somewhat like capillary tubes.

This whole apparatus can be folded up, into a very small compass, under the head and neck. The larynx falls back into the neck, which brings the extreme end of the first portion of the tongue within the upper lip, or behind the two teeth; then the whole of the second part, which consists of five parts, is bent down upon and under this first part, and the two last scales are also bent down over the whole; so that the true tongue is inclosed laterally by the two second horny scales, and over the whole lie the two first.

The œsophagus, in all of this tribe of insects, begins just at the root of the tongue, as in other animals, covered anteriorly by a horny scale, which terminates the head, and which may be called the upper lip, or the roof of the mouth. It passes down through the neck and thorax, and when got into the abdomen, it immediately dilates into a fine transparent bag, which is the immediate receiver of whatever is swallowed. From this the food (whatever it be) is either carried further on into the stomach, to be digested, or is regurgitated for other purposes. To ascertain this in some degree, in living bees, I caught them going out early in the morning, and found this bag quite empty: some time after I caught others returning home, and found the bag quite full of honey, and some of it had got into the stomach. Now I suppose that which was in the craw, was for the purpose of regurgitation; and as probably they had fasted during the night, part had gone on further for digestion. Whatever time the contents of this reservoir may be retained, we never find them altered, so as to give the idea of digestion having taken place: it is pure honey. From this bag the contents can be moved either way; either downwards to the stomach, for the immediate use of the animal itself; or back again, to be thrown out as store for future aliment.

The stomach arises from the lower end, and a little on the right side, of this bag. It does not gradually contract into a stomach, nor is the outlet a passage directly out, but in the centre of a projection which enters some way into the reservoir, being rather an inverted pylorus, thickest at its most projecting part, with a very small opening in the centre, of a peculiar construction. This inward projecting part is easily seen through the coats of the reservoir, especially if full of honey.

The stomach begins immediately on the outside of the reservoir, and the same part which projects into the reservoir, is continued some way into the stomach, but appears to have no particular construction at this end; and therefore it is only fitted to prevent regurgitation into the reservoir, as such would spoil the honey. This construction of parts is well adapted for the purpose; for the end projecting into the reservoir, prevents any honey from getting into the stomach, because it acts there as a valve; therefore whatever is taken in, must be by an action of this vascular part. The stomach has a good deal the appearance of a gut, especially as it seems to come out from a bag. It passes almost directly downwards in the middle of the abdomen. Its inner surface is very much increased, by having either circular valves, somewhat like the *valvulæ conniventes* in the human *jejunum*, or spiral folds, as in the intestine of the shark, &c.; these may be seen through the external coats. In this part the food undergoes the change. Where the stomach terminates, is not exactly to be ascertained; but it soon begins to throw itself into convolutions, and becomes smaller.

The intestine makes two or three twists upon itself, in which part it is enveloped in the ducts, constituting the liver, and probably the pancreas, and at last passes on straight to the termination of the abdomen. Here it is capable of becoming very large, to serve upon occasion as a reservoir, containing a large quantity of excrement: it then contracts a little, and opens under the posterior edge of the last scale of the back, above the sting in the female and labourers, and the penis in the male.

Of the Senses of Bees.

Bees certainly have the five senses. Sight none can doubt. Feeling they also have; and there is every reason for supposing they have likewise taste, smell, and hearing. Taste we cannot doubt: but of smell we may not have such proofs: yet, from observation, I think they give strong signs of smell. When bees are hungry, as a young swarm in wet weather, and are in a glass hive, so that they can be examined, if we put some honey into the bottom, it will immediately breed a commotion; they all seem to be upon the scent: even if they are weak, and hardly able to crawl, they will throw out their probosces as far as possible to get to it, although the light is very faint. This last appears to arise more from smell than seeing. If some bees are let loose in a bee hive, and do not know from which house they came, they will take their stand upon the outside of some hive, or hives; especially when the evening is coming on: whether this arises from the smell of the hives, or sound, I can hardly judge.

Of the Voice of Bees.

Bees may be said to have a voice. They are certainly capable of forming several sounds. They give a sound when flying, which they can vary according to circumstances. One accustomed to bees, can immediately tell when a bee makes an attack, by the sound. These are probably made by the wings. They may be seen standing at the door of their hive, with the belly rather raised, and moving their wings, making a noise. But they produce a noise independent of their wings; for if a bee is smeared all over with honey, so as to make the wings stick together, it will be found to make a noise, which is shrill and peevish. To ascertain this further, I held a bee by the legs, with a pair of pincers; and observed it then made the peevish noise, although the wings were perfectly still: I then cut the wings off, and found it made the same noise. I examined it in water, but it then did not produce the noise, till it was very much teased, and then it made the same kind of noise; and I could observe the water, or rather the surface of contact of the water with the air at the mouth of an air-hole at the root of the wing, vibrating. I have observed that they, or some of them, make a noise the evenings before they swarm, which is a kind of ring, or sound of a small trumpet: by comparing it with the notes of the piano forte, it seemed to be the same with the lower A of the treble.

Of the Female Parts.

I may here observe, that insects differ from most of the classes of animals above them, in having their eggs formed in the ducts along which they pass; not in a cluster on the back, as in some fish (for instance all of the ray kind, or what are called the amphibia), in the bird, and as is supposed in the quadruped; from thence the eggs are taken up, and by the ducts are carried along to their places of destination.

Of the Oviducts.

The female of the common bee, similar to all the females of the bee tribe, has six oviducts on each side, beginning by very small, and almost imperceptible threads, as high as the chest: they then form one cord coiled up, or pass very serpentine, and become larger and larger as they approach the anus, owing to the gradual increased size of the eggs in them, which are now more distinct, and give the duct a sort of interrupted appearance, toward the lower end. The six ducts, when full of eggs, make a kind of quadrangle; then all unite into one duct, which enters the duct common to it and the oviducts of the other side. The ducts common to the six oviducts on each side, are extremely tender; so much so, that it is difficult to save them. The duct common to those on both sides may be called the vagina, and it is continued to the anus, or termination of the belly.

Of the Male Parts.

The male parts of generation, in the common bee, are much larger than in the humble bee. This we suppose necessary, considering the vast number of eggs the common bee lays, more than the humble bee does.

The external parts of generation of the male bee, are rather more under the belly, than in the others of this tribe; not so much at the termination of the belly; and they are rather more exposed, the two last scales, especially the under one, not projecting so much: the two holders are not so projecting beyond their base, nor are they so hooked, or sharp, as in the humble bee; hardly deserving the name of holders. From the external parts, passes up into the abdomen a pretty large sheath, whose termination incloses the glans penis. It is a bulbous part, having a dark coloured horny part upon it, which has two processes near its opening externally, one on each side, of a yellow colour: it has another process, which is white, and seems to be a gland. It can be made to pass along this sheath, or prepuce, and appear externally: I have been able, with a pair of forceps, to invert the sheath, beginning externally at the mouth, and pulling out a little at a time, by shifting my hold, till the glans has appeared externally.

The internal parts are the testicles, with their appendages. The testicles are two small oblong bodies, lying near the back, having a vast number of air-vessels passing into them, and ramifying upon them. They are of a pale yellowish colour. From their lower ends pass down ducts, which may be called *vasa deferentia*, and which enter two bags: these two bags, into which the *vasa deferentia* enter, are probably reservoirs for the semen. From the union of these two bags passes out a duct, which runs towards the termination of the abdomen, and ends in the penis. These three parts, namely, testicles with their ducts, the two bags, and the duct arising from them, which I have termed urethra, are all folded on each other, so as to appear as one body.

In the introduction to this account of bees I observed, that several things in their œconomy might escape us if we considered them alone, but might be made out in other insects: an instance of this occurs in the impregnation of the female bee. The death of the males in the month of August, so that not one is left, and yet the queen to breed in the month of March, must puzzle any one not acquainted with the mode of impregnation of the females of most insects. Insects, respecting the males, are of two kinds: one, where the male lives through the winter, as well as the female; and the other, where every male of that species dies before the winter comes on; among which may be considered, as a third, those where both male and female die the same year. Of the first, I shall only give the common fly as an instance; of the second, I shall just mention all of the bee tribe; and the third may be illustrated in the silk-worm. The mode of impregnation in the first, is its being continued uninterruptedly through the whole period of laying eggs; while in the second, the copulation is in store; and, in the third the female lays up, by the copulation, a store of semen, although the male is alive: of this I shall now give an explanation in the silk-moth, which may be applied to the bee, and many other insects.

In dissecting the female parts in the silk-moth, I discovered a bag lying on what may be called the vagina, or common oviduct, whose mouth, or opening, was external, but it had a canal of communication between it and the common oviduct. In dissecting these parts before copulation, I found this bag empty, and when I dissected them after, I found it full. Suspecting this to contain the semen of the male, I immediately conceived the following experiment: I opened the female as soon as the male had united to her, and found the penis in the opening of this bag, and by opening the duct where the penis lay, I observed the semen lying on the end of the penis. In another, I observed the bag to fill in the time of copulation: and in a pair that died in the act, I found the penis in this passage.

When we consider the impregnation of the egg in the silkworm, we may observe the following circumstances:

First, many of the ova are completely formed, and covered with a hard shell, before copulation: secondly, the animals are a vast while in the act of copulation: and thirdly, the bags at the anus are filled during the time of copulation. From the first observation it appears, that the egg can receive the male influence through the hard or horny part of the shell. To know how far the whole, or only a part of the eggs, were impregnated by each copulation, I made the following experiments.* I took a female just emerged out of her cell, and put a male to her, and allowed them to be connected their full time. They were in copulation ten hours. I then put her into a box by herself, and when she laid her eggs, I numbered the different parcels as she laid them, viz. 1, 2, 3, 4, 5; these eggs I preserved, and in the summer following I perceived that the No. 5 was as prolific as the No. 1; so that this one copulation was capable of impregnating the whole brood: and therefore the male influence must go either along the oviduct its whole length, and impregnate the incomplete eggs, as well as the complete, which appears to me not likely; or those not yet formed were impregnated from the reservoir in the act of laying: for I conceived that these bags, by containing semen, had a power of impregnating the egg as it passed along to the anus, just as it traversed the mouth of the duct of communication.

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* All these experiments on the silk-moth were begun in the summer 1767, and repeated by Mr. BELL in the year 1770.

Finding that eggs completely formed, could be impregnated by the semen, and also finding that the before-mentioned bag was a reservoir for the semen till wanted, I wished next to discover if they could be impregnated from the semen of this bag; but as this must be done without the act of copulation, I conceived it proper, first, to see whether the ova of insects might be impregnated without the natural act of copulation, by applying the male semen over the ova, just as they were laid. The following experiments were made on the silk-moth:

EXPERIMENT I.

I took a female moth, as soon as she escaped from her pod, and kept her carefully by herself, upon a clean card, till she began to lay; then I took males that were ready for copulation, opened them, exposing their seminal ducts, and after cutting into these, collected their semen with a hair pencil: with this semen I covered the ova, as soon as they passed out of the vagina. The card with these eggs, having a written account of the experiment upon it, I kept in a box by itself. In the ensuing season, eight of the ova hatched at the same time with others naturally impregnated. Thus, then, I ascertained that the eggs could be impregnated by art, after they were laid.

The ova laid by females that had not been impregnated, did not stick where they were laid: so that the semen would appear not only to impregnate the ova, but also to be the means of attaching them.

To know whether that bag in the female silk-moth, which increased at the time of copulation, was filled with the semen of the male, I made the following experiment:

EXPERIMENT II.

I took a female moth, as soon as she had escaped from the pod, and kept her on a card till she began to lay. I then took females that were fully impregnated before they began to lay, and dissected out that bag which I supposed to be the receptacle for the male semen; and wetting a camel hair pencil with this matter, covered the ova as soon as they passed out of the vagina. These ova were laid carefully on the clean card, and kept till the ensuing season, when they all hatched at the same time with those naturally impregnated.

This proves that this bag is the receptacle for the semen, and gradually decreases as the eggs are laid.

Of the Sting of the Bee.

I have observed that it is only the queen and the labourers that have stings; and this provision of a sting is perhaps as curious a circumstance as any attending the bee, and probably is one of the characters of the bee tribe.

The apparatus itself is of a very curious construction, fitted for inflicting a wound, and at the same time conveying a poison into that wound. The apparatus consists of two piercers, conducted in a groove, or director, which appears to be itself the sting. This groove is somewhat thick at its base, but terminates in a point; it is articulated to the last scale of the upper side of the abdomen by thirteen thin scales, six on each side, and one behind the rectum. These scales inclose, as it were, the rectum or anus all round; they can hardly be said to be articulated to each other, only attached by thin membranes, which allow of a variety of motions; three of them, however, are attached more closely to a round and curved process, which comes from the basis of the groove in which the sting lies, as also to the curved arms of the sting, which spread out externally. The two stings may be said to begin by those two curved processes at their union with the scales, and converging towards the groove at its base, which they enter, then pass along it to its point. They are serrated on their outer edges, near to the point. These two stings can be thrust out beyond the groove, although not far, and they can be drawn within it; and, I believe, can be moved singly. All these parts are moved by muscles, which we may suppose are very strong in them, much stronger than in other animals;

and these muscles give motion in almost all directions, but more particularly outwards. It is wonderful how deep they will pierce solid bodies with the sting. I have examined the length they have pierced the palm of the hand, which is covered with a thick cuticle: it has often been about the $\frac{1}{12}$ of an inch. To perform this by mere force, two things are necessary, power of muscles, and strength of the sting; neither of which they seem to possess in sufficient degree. I own I do not understand this operation. I am apt to conceive there is something in it distinct from simple force applied to one end of a body; for if this was simply the case, the sting of the bee could not be made to pierce by any power applied to its base, as the least pressure bends it in any direction: it is possible the serrated edges may assist, by cutting their way in, like a saw.

The apparatus for the poison consists of two small ducts, which are the glands that secrete the poison: these two lie in the abdomen, among the air-cells, &c.: they both unite into one, which soon enters into, or forms, an oblong bag, like a bladder of urine; at the opposite end of which passes out a duct, which runs towards the angle where the two stings meet; and entering between the two stings, is continued between them in a groove, which forms a canal by the union of the two stings to this point. There is another duct on the right of that described above, which is not so circumscribed, and contains a thicker matter, which, as far as I have been able to judge, enters along with the other: but it is the first that contains the poison, which is a thin, clear fluid. To ascertain which was the poison, I dipped points of needles into both, and pricked the back of the hand; and those punctures that had the fluid from the first-described bags in them grew sore and inflamed, while the others did not. From the stings having serrated edges, it is seldom the bees can disengage them; and they immediately upon stinging endeavour to make their escape, but are generally prevented, as it were caught in their own trap; and the force they use commonly drags out the whole of the apparatus for stinging, and also part of the bowels; so that the bee most frequently falls a sacrifice immediately upon having effected its purpose. Upon a superficial view, one conceives, that the first intention of the bee having a sting is evident; one sees it has property to defend, and that therefore it is fitted for defence; but why it should naturally fall a sacrifice in its own defence, does not so readily appear: besides, all bees have stings, although all bees have not property to defend, and therefore are not under the same necessity of being so provided. Probably its having a sting to use, was sufficient for nature to defend the bee, without using it liberally; and the loss of a bee or two, when they did sting, was of no consequence; for it is seldom that more die.

I have now carried the operations of a hive, or the œconomy of the bee, completely round the year; in which time they revolve to the first point we set out at, and the continuance is only a repetition of the same revolutions as I have now described: but those revolutions occasion a series of effects in the comb, which effects in time produce variations in the life of the hive. Besides, there are observations that have little to do with the œconomy of a year, but include the whole of the life of this insect, or at least its hive.

Of the Life of the Bee.

I have observed that the life of the male is only one summer, or rather a month or two; and this we know from there being none in the winter, otherwise their age could not be ascertained, as it is impossible to learn the age of either the queen or labourers. Some suppose that it is the young bees which swarm; and most probably it is so: but I think it is probable also, that a certain number of young ones may be retained to keep up the stock, as we must suppose that many of the old ones are, from accidents of various kinds, lost to the hive; and we could conceive, that a hive three or four years old might not have an original bee in it, although a bee might live twice that time. But there must be a period for a bee to live; and if I were to judge from analogy, I should say, that a bee's natural life is limited to a certain number of seasons; viz. one bee does not live one year, another two, another three, &c. I even conceive that no individual insect of any species lives one month longer than the others of the same species. I believe this is the case with all insects; but the age of either a labourer or a queen may never be discovered. One might suppose that the life of a bee, and the time a hive can possibly last, would be nearly equal: although this is not absolutely necessary, because they can produce a succession, which they probably do; for I am very ready to imagine, that after the first brood in the season, all the last winter bees die, and the hive is occupied with this first brood; and that they breed the first swarm, or that the old breed the whole of this season's breeding, and then die, and those that continue through the winter are the young; and if so, then they follow the same course with their progenitors.

The comb of a hive may be said to be the furniture and storehouse of the bees, which by use wear out; and from the description I have given, it will appear that the comb in time will be rendered unfit for use. I observed, that they did not clean out the excrement of the maggot, and that the maggot, before it moved into the chrysalis state, lined the cell with a silk, similar to many other insects. It lines the whole cell, top, sides, and bottom; the two last are permanent; and at the bottom it covers with this lining its own excrement.* Why the bee maggot is formed to do this, is, probably, because honey afterwards is to be put into this cell; so that the honey is laid into this last silken bag. How often they may breed in the same cell I do not know, but I have known them three times in the same season; each time the excrement has been accumulating, and the cell has been lined three times with silk. From this account we must see that a cell, in time, will be so far filled up as to render it unfit for breeding. On separating the lining of silk, which is easiest done at the bottom, on account of the dried excrement between each lining, I have counted above twenty different linings in one cell, and found the cell about one quarter, or one third, filled up: when such a cell, or a piece of comb with such cells, is steeped in water, so as to soften the excrement between the linings, they are separated from each other at the bottom by the swelling of the excrement, so that they can be easily counted. A piece of comb so circumstanced, when boiled for the wax, will keep its form, and the small quantity of wax is squeezed out at different parts, as if squeezed out of a sponge, and runs together into the crevices: while a piece of comb, that never has been bred in, even of the same hive, melts almost wholly down. It is this wax that has the fine yellow, while the other of the same hives, although brown, yet shall be white when melted; so that I was led to imagine the wax took its tinge from the farina, excrement, &c. but upon boiling pure wax with such materials, it was not tinged with this transparent yellow, only became dirty. In some of those cells that had probably been bred in twenty times, or more, when soaked so as to make the excrement swell, I have seen the bottom of the last lining rise even with the mouth, or top of the cell, so that the cavity of the cell was now full: in others, I have seen it rise higher than the mouth, so that the last formed layers were almost inverted, and turned inside out. A piece of such comb, consisting of two rows of cells, is to be considered as a mould, and the lining of silk and the excrement as the cast; when this is boiled, so as either to extract all the wax or mould, or to destroy its original regular formation which constituted the comb, and nothing is left but the cells of silk, &c. they all easily separate from each other, being only so many casts, with the mould destroyed; and the bottoms, which were indented into each other, are very perfect.

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* This neither the wasp nor hornet do, although they do not clean out the excrement of their maggots.

From the above account we must see that the combs of a hive can only last a certain number of years; however, to make them last longer, the bees often add a little to the mouth of the cell, which is seldom done with wax alone, but with a mixture; and they sometimes cover the silk lining of the last chrysalis; but all this makes such cells clumsy, in comparison to the original ones.

To CHARLES BLAGDEN, *M.D. Sec. R.S.*

DEAR SIR,

IN a Paper on the *Chronology* of the HINDOOS, which the Society did me the honour to print in the LXXXth Volume of their Transactions, I have said (p. 566 and 570) that the era of *Bikramajit* commences in the year 56 before Christ, and that the year 1846 corresponds with our year 1790 (from April): but it appears from an examination of the numbers of an original *Benares* almanack, that I have calculated the elapsed time of that era one year short (by comparison with the era of *Salabân*, which being employed in astronomical computations is sufficiently ascertained), and that the year 1847, not 1846, should correspond with 1790, and the commencement of the era with the year 57 before Christ, both from April.

I am very desirous that this error should be noticed as soon as possible, and request that the correction may have a place in the Volume now printing.

I am, dear Sir, &c.

WM. MARSDEN.

METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

for January, 1791.

1791	Time.		Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inches.	Points.	Str.	
Jan. 1	8	0	38,5	48,5	30,03		SSW	1	Cloudy.
	2	0	43	51,5	29,91		SSW	1	Cloudy.
2	8	0	33	48,5	29,64	0,090	WSW	1	Cloudy.
	2	0	43	49	29,30		SSW	1	Cloudy.
3	8	0	31	46	29,30	0,070	W	1	Fine.
	2	0	36,5	51	29,37		W	1	Fine.
4	8	0	35	48,5	29,28	0,025	SE	1	Fine.
	2	0	38	50,5	28,91		SE	2	Rain.
5	8	0	34	48	29,17	0,015	SW	2	Cloudy.
	2	0	39	51	29,26		SW	2	Rain.
6	8	0	34	48,5	29,37	0,010	W	2	Fine.
	2	0	42	53	29,40		WSW	2	Fine.
7	8	0	48	50	29,05	0,251	WSW	2	Cloudy; much wind
	2	0	49	55	29,05		W	2	Fair. [last night.
8	8	0	41	50,5	29,11	0,023	WNW	2	Fair; much wind last
	2	0	44	54	29,24		WNW	2	Fair. [night.
9	8	0	41	50	28,78	0,070	W	2	Rain; much wind last
	2	0	43	52	28,96		WNW	2	Cloudy. [night.
10	8	0	39	50,5	29,71		SW	1	Cloudy.
	2	0	49	55	29,68		SSW	1	Cloudy.
11	8	0	48	54	29,68	0,205	W	1	Rain; much wind last
	2	0	48	56	29,69		W	1	Cloudy. [night.
12	8	0	40	53	29,94		SW	1	Fair.
	2	0	46	56	29,78		SSW	2	Cloudy.
13	8	0	43	53	29,26	0,280	W	2	Cloudy.
	2	0	45	56	29,38		WNW	2	Fair.
14	8	0	40,5	53	29,78	0,050	W	1	Cloudy.
	2	0	47	55	29,58		SW	2	Cloudy.
15	8	0	34,5	53	30,01	0,040	W	1	Fine.
	2	0	43,5	56	29,94		SW	2	Cloudy.
16	8	0	47	53,5	29,82	0,016	SW	2	Cloudy.
	2	0	53	58	29,77		WSW	2	Cloudy.

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for January, 1791.

1791	Time.		Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inches.	Points.	Str.	
Jan. 17	8	0	47	54	29,40		W	2	Cloudy.
	2	0	48	57	29,45		W	2	Cloudy.
18	8	0	40	53,5	29,47		S	2	Cloudy.
	2	0	43	57	29,10		S	2	Rain.
19	8	0	42	52,5	28,68	0,193	SSW	2	Cloudy.
	2	0	43	56,5	28,68		SW	2	Cloudy.
20	8	0	41	52,5	28,18	0,197	WSW	2	Cloudy.
	2	0	43	56	28,46		W	2	Fine.
21	8	0	37	53	29,08	0,160	WNW	2	Fine.
	2	0	42	56	29,32		WNW	2	Fine.
22	8	0	35	52	29,86		W	1	Fair.
	2	0	43	54,5	29,76		SSW	2	Cloudy.
23	8	0	46	54,5	29,71	0,101	W	1	Cloudy.
	2	0	46,5	57	29,88		WNW	1	Cloudy.
24	8	0	34,5	52	30,50		W	1	Fine.
	2	0	42	55	30,48		W	1	Cloudy.
25	8	0	41	52,5	30,27		W	1	Cloudy.
	2	0	47	54	30,20		W	2	Cloudy.
26	8	0	43	53,5	30,15		W	1	Cloudy.
	2	0	45,5	54	30,11		W	1	Cloudy.
27	8	0	41,5	53	29,80		W	1	Rain.
	2	0	42	54	29,73		WNW	1	Rain.
28	8	0	34	51,5	29,98	0,150	NW	1	Fair.
	2	0	36,5	54,5	30,08		NW	2	Fine.
29	8	0	37	48,5	29,95		W	1	Cloudy.
	2	0	43,5	51	29,85		W	1	Cloudy.
30	8	0	41	51	29,77	0,011	W	1	Cloudy.
	2	0	47	53	29,85		NW	1	Fair.
31	8	0	47,5	52	29,83		WNW	2	Fair.
	2	0	52	55	29,89		WNW	2	Fair.

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for February, 1791.

1791	Time.		Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inches.	Points.	Str.	
Feb. 1	7	0	45	53	29,82		W	1	Cloudy.
	2	0	49	55	29,63		W	2	Fair.
2	7	0	37	52	29,94		WNW	2	Fair.
	2	0	39	54	30,11		WNW	2	Fine.
3	7	0	30	49	30,20		NNW	2	Fine.
	2	0	37	51	30,28		NW	2	Fine.
4	7	0	30,5	49	30,44		NNW	2	Fine.
	2	0	38	51,5	30,43		NNW	2	Fair.
5	7	0	37	49,5	30,48		W	1	Foggy.
	2	0	42	52	30,44		WNW	1	Cloudy.
6	7	0	34	49,5	30,40		W	1	Cloudy.
	2	0	40,5	51,5	30,40		W	1	Cloudy.
7	7	0	41	50,5	30,33		W	1	Foggy.
	2	0	48	53	30,33		W	1	Cloudy.
8	7	0	41,5	51,5	30,38		W	1	Foggy.
	2	0	48	54,5	30,37		W	1	Cloudy.
9	7	0	41,5	52	30,36		W	1	Cloudy.
	2	0	47	54	30,28		W	1	Cloudy.
10	7	0	44	52	30,08		W	2	Cloudy.
	2	0	47,5	54	30,03		SW	2	Cloudy.
11	7	0	47	54	29,76		SW	2	Cloudy.
	2	0	47,5	57	29,61		SW	2	Cloudy.
12	7	0	35	54	30,10		W	2	Fine.
	2	0	45	56	30,20		W	2	Fine.
13	7	0	42	53	29,94	0,265	W	2	Rain.
	2	0	48,5	55	29,86		WSW	2	Rain.
14	7	0	49,5	55	29,86	0,033	WSW	2	Cloudy.
	2	0	52,5	56,5	29,85		WSW	2	Cloudy.
15	7	0	46	55	29,76	0,032	WSW	1	Cloudy.
	2	0	48	56,5	29,78		WSW	2	Cloudy.
16	7	0	39,5	53,5	29,84		W	2	Fair.
	2	0	46,5	55,5	30,01		W	1	Fair.

METEOROLOGICAL JOURNAL
for February, 1791.

1791	Time.		Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inches.	Points.	Str.	
Feb. 17	7	0	32,5	53	30,21		SW	1	Fair.
	2	0	44	53,5	29,98		SW	2	Cloudy.
18	7	0	36,5	52,5	29,27	0,035	WSW	1	Fine.
	2	0	42	53,5	29,16		WSW	1	Fair.
19	7	0	34,5	51,5	29,30		E	1	Rain.
	2	0	40	54,5	29,56		NNW	1	Cloudy.
20	7	0	35,5	51	29,78	0,057	NE	1	Fine.
	2	0	42	53,5	29,67		NE	1	Cloudy.
21	7	0	38,5	51	29,52	0,096	E	1	Cloudy.
	2	0	43	53,5	29,54		S	1	Cloudy.
22	7	0	35,5	50	29,70		SE	1	Fine.
	2	0	47	53	29,74		S by W	1	Fair.
23	7	0	45	50,5	29,52	0,026	S	2	Cloudy.
	2	0	50	54	29,37		SSW	2	Cloudy.
24	7	0	33,5	51	29,86	0,089	WNW	1	Fine.
	2	0	43	54,5	29,98		WNW	1	Fine.
25	7	0	38	51	29,96		E	1	Cloudy.
	2	0	41	53	29,85		E	1	Rain.
26	7	0	33	48,5	29,68	0,240	NNE	2	Snow.
	2	0	35	51,5	29,80		N by E	2	Rain.
27	7	0	34,5	48,5	29,97		N	1	Fair.
	2	0	41	52	29,97		N	2	Fair.
28	7	0	34	49	30,06		N	2	Fair.
	2	0	41	51,5	30,08		NW	2	Fair.

METEOROLOGICAL JOURNAL
for March, 1791.

1791	Time.		Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inches.	Points.	Str.	
Mar. 1	7	0	32	48	30,13		NNW	1	Cloudy.
	2	0	39	51	30,12		N	1	Fair.
2	7	0	32,5	48,5	30,24		N	1	Cloudy.
	2	0	39	52	30,27		NE	1	Hazy.
3	7	0	31,5	48	30,51		WNW	1	Fair.
	2	0	41	53	30,51		W	1	Fine.
4	7	0	42	50	30,46		W	1	Cloudy.
	2	0	52	56	30,47		W	1	Fine.
5	7	0	45	52	30,45		W	1	Cloudy.
	2	0	48	55	30,40		WNW	1	Cloudy.
6	7	0	37,5	51,5	30,24		WNW	1	Hazy.
	2	0	48	55	30,20		WNW	1	Hazy.
7	7	0	38	51,5	30,27		WNW	1	Fair.
	2	0	47	54,5	30,37		NNW	2	Fair.
8	7	0	33,5	50,5	30,58		NNW	2	Fine.
	2	0	43	54	30,63		NNW	2	Fine.
9	7	0	32,5	50	30,67		W	1	Fine.
	2	0	45	54,5	30,62		W	1	Fine.
10	7	0	32,5	51,5	30,60		W	1	Hazy.
	2	0	50,5	54,5	30,54		WNW	1	Fair.
11	7	0	37	53	30,47		W	1	Hazy.
	2	0	48,5	54	30,44		WNW	1	Hazy.
12	7	0	37,5	53	30,28		WNW	1	Hazy.
	2	0	50	56,5	30,21		W	1	Fine.
13	7	0	39	53,5	30,18		W	1	Fine.
	2	0	53	58	30,16		W	1	Fine.
14	7	0	42	54	30,14		W	1	Fair.
	2	0	54	57,5	30,25		WNW	1	Cloudy.
15	7	0	46	54	30,36		W	2	Cloudy.
	2	0	55	59,5	30,37		WSW	2	Fine.
16	7	0	48	57	30,44		W	2	Fair.
	2	0	55	61,5	30,44		WSW	2	Fine.

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1791	Time.		Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inches.	Points.	Str.	
Mar. 17	7	0	38	57	30,41		W	1	Cloudy.
	2	0	49	60	30,40		W	1	Fine.
18	7	0	38	58	30,28		W	1	Fair.
	2	0	55	59,5	30,30		W	1	Cloudy.
19	7	0	38	56,5	30,18		W	1	Fair.
	2	0	51,5	61	30,02		WSW	2	Fair.
20	7	0	46	55,5	29,60		SW	2	Cloudy.
	2	0	49	59,5	29,44		SW	2	Rain.
21	7	0	41	55	28,90	0,458	WNW	2	Cloudy.
	2	0	48	58,5	29,26		NW	2	Fair.
22	7	0	40	55	29,83		WNW	2	Fair.
	2	0	51,5	58,5	29,99		WNW	2	Fair.
23	7	0	50	57	30,05		W	2	Cloudy.
	2	0	55	62	30,14		W	2	Fair.
24	7	0	38,5	57	30,53		W	1	Hazy.
	2	0	50	59,5	30,51		NW	1	Fine.
25	7	0	40	56,5	30,43		W	1	Fair.
	2	0	50	60	30,30		SW	2	Fine.
26	7	0	44	57	29,83		E	1	Cloudy.
	2	0	44	58,5	29,75		W	1	Rain.
27	7	0	39,5	56,5	29,98	0,258	NNW	1	Cloudy.
	2	0	46	60	30,05		N	1	Cloudy.
28	7	0	42	53	30,12		WNW	1	Cloudy.
	2	0	45,5	58,5	30,11		WNW	1	Cloudy.
29	7	0	47	56,5	30,04		W	1	Cloudy.
	2	0	49	59	30,02		NE	1	Cloudy.
30	7	0	43	56	30,01		E	1	Cloudy.
	2	0	49	58	30,01		E	1	Cloudy.
31	7	0	43	55	30,08		E	1	Cloudy.
	2	0	51,5	58	30,08		E	1	Cloudy.

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1791	Time.		Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inches.	Points.	Str.	
April 1	7	0	44	55,5	29,99		SW	1	Cloudy.
	2	0	52	58,5	29,93		SSW	1	Fair.
2	7	0	43	56	29,78		E	1	Fair.
	2	0	53	60,5	29,72		ESE	2	Fine.
3	7	0	45	57	29,75		E	2	Fine.
	2	0	55	61	29,80		E	2	Fine.
4	7	0	41	57	29,88		ENE	1	Fine.
	2	0	51	60	29,84		ENE	2	Fine.
5	7	0	43	57	29,70		ENE	1	Cloudy.
	2	0	47	59,5	29,60		ENE	1	Cloudy.
6	7	0	42	57	29,38	0,102	NE	1	Rain.
	2	0	42,5	57,5	29,44		W	1	Rain.
7	7	0	45	57,5	29,74	0,478	W	1	Fair.
	2	0	54	59	29,83		W	2	Fair.
8	7	0	47	57,5	29,93		E	1	Cloudy.
	2	0	61	59,5	29,90		SE	1	Cloudy.
9	7	0	49	58	29,90		W	1	Cloudy.
	2	0	60	60,5	29,86		W	1	Fair.
10	7	0	47	57,5	29,96		W	1	Fair.
	2	0	58	61,5	29,98		W	1	Fair.
11	7	0	49	58,5	29,98	0,037	E	1	Cloudy.
	2	0	59	60,5	29,94		E	1	Cloudy.
12	7	0	45	58	29,95		W	1	Fair.
	2	0	60	62	29,97		W	1	Fair.
13	7	0	49	58,5	29,98		E	1	Cloudy.
	2	0	56	62	29,93		E	1	Cloudy.
14	7	0	53	60	29,99	0,170	W	1	Fair.
	2	0	59	63	30,01		W	1	Fine.
15	7	0	49,5	59,5	30,08		W	1	Fine.
	2	0	61	63	30,08		W	1	Hazy.
16	7	0	51	59	30,11		E	1	Fair.
	2	0	60	64	30,10		E	1	Fine.

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1791	Time.		Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inches.	Points.	Str.	
Apr. 17	7	0	53	61,5	30,00		E	1	Fine.
	2	0	64	64	29,93		ESE	1	Fine.
18	7	0	52	60,5	29,77		SW	2	Cloudy.
	2	0	61	64	29,73		WNW	1	Fair.
19	7	0	49,5	60,5	29,69		WSW	2	Fair.
	2	0	61	63	29,62		WSW	2	Cloudy.
20	7	0	51	59,5	29,50	0,013	SW	2	Cloudy.
	2	0	59	62	29,49		SW	2	Cloudy.
21	7	0	52,5	61,5	29,40		WSW	1	Fair.
	2	0	61	62,5	29,34		SW	1	Cloudy.
22	7	0	51	64	29,14	0,105	SW	1	Rain.
	2	0	58	63,5	29,08		WSW	1	Cloudy.
23	7	0	48	58	29,15	0,090	WSW	2	Fine.
	2	0	55	62	29,16		WSW	2	Fair.
24	7	0	49	59,5	29,26	0,130	WSW	2	Fine.
	2	0	55	61,5	29,36		WSW	2	Fair.
25	7	0	47	59,5	29,83		WNW	1	Fine.
	2	0	52	61	29,89		WNW	2	Rain.
26	7	0	46	58	30,02	0,153	NW	1	Hazy.
	2	0	58	62	29,98		E	2	Hazy.
27	7	0	49	59	29,85	0,142	E	1	Cloudy.
	2	0	55	60	29,83		NE	1	Cloudy.
28	7	0	50	59	29,80		NW	1	Cloudy.
	2	0	58	60,5	29,82		W	1	Cloudy.
29	7	0	48	60	29,85	0,040	N	1	Cloudy.
	2	0	52	59	29,92		N	1	Cloudy.
30	7	0	46	58	29,95		N	2	Cloudy.
	2	0	45	59	29,91		N	2	Cloudy.

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1791	Time.		Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inches.	Points.	Str.	
May 1	7	0	45	58,5	29,81	0,061	N	1	Cloudy.
	2	0	48	58	29,77		N	1	Cloudy.
2	7	0	47	58	29,86	0,357	N	1	Rain.
	2	0	49	58,5	29,91		NNE	2	Cloudy.
3	7	0	43,5	57	30,01	0,086	NNE	2	Cloudy.
	2	0	48	58	30,01		N	2	Cloudy.
4	7	0	43	56	30,00		N	1	Cloudy.
	2	0	45	58	30,00		N	2	Cloudy.
5	7	0	43	55	29,95		N	1	Cloudy.
	2	0	51	57,5	29,95		N	2	Fair.
6	7	0	42	55,5	30,00	0,115	NE	2	Fair.
	2	0	49	57	30,07		NNE	2	Fair.
7	7	0	39	55	30,30		N	1	Fine.
	2	0	50,5	57	30,33		N	1	Cloudy.
8	7	0	43	55	30,37		W	1	Fine.
	2	0	57	58,5	30,30		NW	1	Fair.
9	7	0	49	56	30,15		W	1	Cloudy.
	2	0	60	58	30,09		W	1	Cloudy.
10	7	0	51,5	57,5	30,02		W	1	Cloudy.
	2	0	55	58,5	30,06		N	1	Cloudy.
11	7	0	47	57	30,08	0,065	W	1	Hazy.
	2	0	57	58	29,96		S	1	Cloudy.
12	7	0	49,5	57	29,72	0,070	W	1	Cloudy.
	2	0	54	57,5	29,63		SSE	1	Cloudy.
13	7	0	49,5	56,5	29,59		SSW	1	Fine.
	2	0	59	58	29,64		SSW	1	Cloudy.
14	7	0	50,5	57	29,95		W	1	Fine.
	2	0	62	59	30,02		WSW	1	Fine.
15	7	0	52	57,5	30,16		W	1	Fine.
	2	0	65,5	59,5	30,11		WSW	1	Cloudy.
16	7	0	55	58	30,00		W	1	Cloudy.
	2	0	60	59	30,02		W	1	Fair.

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1791	Time.	Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H. M.	°	°	Inches.	Inches.	Points.	Str.	
May 17	7 0	51	58,5	29.87		W	2	Cloudy.
	2 0	61	60	29,80		WNW	2	Cloudy.
18	7 0	49	58	29,86		W	2	Cloudy.
	2 0	58	60	29,92		W	2	Fair.
19	7 0	53	58	29,55		W	2	Cloudy.
	2 0	57	59	29,53		W	2	Cloudy.
20	7 0	50	57·5	29,97		W	2	Fine.
	2 0	58	58,5	29,96		W	2	Cloudy.
21	7 0	50	57·5	30,03		W	2	Cloudy.
	2 0	61	59	29,96		W	2	Fine.
22	7 0	50	58,5	29,94		W	2	Fair.
	2 0	60	60	29,97		W	2	Fair.
23	7 0	50,5	58	29,85		S	2	Cloudy.
	2 0	59	58,5	29,69		SW	2	Cloudy.
24	7 0	49	55	29,92	0,040	WSW	1	Fine.
	2 0	58	58	29,97		WNW	2	Cloudy.
25	7 0	49	57	30,11		SW	1	Cloudy.
	2 0	56,5	58	30,14		N	1	Cloudy.
26	7 0	50	57·5	30,32		ENE	1	Fine.
	2 0	60	58,5	30,30		NE	2	Fine.
27	7 0	53	58	30,33		N	1	Fine.
	2 0	66	60	30,30		E	1	Cloudy.
28	7 0	53	58	30,34		ENE	1	Fine.
	2 0	67,5	61,5	30,29		E	1	Fine.
29	7 0	51	60	30,27		NE	1	Cloudy.
	2 0	63	61,5	30,19		NNE	1	Fine.
30	7 0	51	58	30,18		NE	1	Cloudy.
	2 0	62,5	61,5	30,16		NE	1	Fair.
31	7 0	52	60	30,16		E	1	Cloudy.
	2 0	61	61,5	30,10		E	1	Fine.

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1791	Time.		Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inches.	Points.	Str.	
June 1	7	0	56	61	30,07		E	1	Hazy.
	2	0	69,5	64,5	30,03		SSW	1	Fine.
2	7	0	60	62,5	30,08		SW	1	Fine.
	2	0	79	66	30,08		S by E	1	Fair.
3	7	0	60	64	30,10		E	1	Fine.
	2	0	75,5	67	30,11		E	1	Fine.
4	7	0	63	66	30,10		E	1	Fine.
	2	0	76	68,5	30,10		E	1	Fine.
5	7	0	65	67	30,09		E	1	Fine.
	2	0	79,5	69	30,05		E	1	Fine.
6	7	0	63	67,5	30,13		ENE	1	Fine.
	2	0	79,5	69,5	30,15		E	1	Fine.
7	7	0	63	67,5	30,22		ENE	1	Fine.
	2	0	80	70,5	30,22		NNE	1	Fine.
8	7	0	64	69	30,14		NE	1	Cloudy.
	2	0	66	69	30,12		NNE	2	Cloudy.
9	7	0	53,5	66,5	30,05		NNE	2	Fair.
	2	0	67	68,5	30,01		N	2	Fine.
10	7	0	56	65	29,90		WNW	1	Fine.
	2	0	69	67	29,85		NW	1	Cloudy.
11	7	0	49	66	29,68		E	2	Cloudy.
	2	0	58	64	29,72		NW	2	Cloudy.
12	7	0	47	62	29,79		WNW	1	Cloudy.
	2	0	52	62	29,75		N	2	Fair.
13	7	0	47	60	29,79		NW	1	Fair.
	2	0	56	61	29,78		N	2	Fair.
14	7	0	47	59	29,81	0,046	NNW	1	Fine.
	2	0	57,5	59,5	29,79		N	1	Cloudy.
15	7	0	49	58,5	29,83		NNE	2	Fine.
	2	0	56	58,5	29,82		NNE	1	Cloudy.
16	7	0	48	58	29,44	0,058	N	2	Rain.
	2	0	58	59,5	29,39		N E	2	Cloudy.

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1791	Time.		Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inches.	Points.	Str.	
June 17	7	0	53	58,5	29,67	0,075	E	2	Cloudy.
	2	0	59,5	59	29,75		E	2	Cloudy.
18	7	0	52	59	29,79		NE	1	Cloudy.
	2	0	58	59,5	29,74		NE	1	Cloudy.
19	7	0	53	59	29,74	0,020	NE	2	Fine.
	2	0	64	61	29,72		NE	2	Fine.
20	7	0	50	59	29,74		NE	2	Cloudy.
	2	0	58	59	29,79		NE	1	Cloudy.
21	7	0	51	58,5	29,85		WNW	2	Cloudy.
	2	0	57	58	29,84		SW	2	Cloudy.
22	7	0	47,5	58	29,92	0,133	W	1	Fair.
	2	0	59	58,5	29,92		W	2	Fair.
23	7	0	50	57	29,95		W	2	Fine.
	2	0	65	58,5	30,00		W	2	Cloudy.
24	7	0	56	59	30,18		W	2	Fine.
	2	0	70,5	60	30,19		W	2	Fair.
25	7	0	60	60	30,22		W	1	Fair.
	2	0	72	62	30,16		W	2	Fair.
26	7	0	62	62	30,08		W	2	Fair.
	2	0	76	64	30,00		SW	2	Fair.
27	7	0	61	64	29,91		W	1	Fair.
	2	0	72,5	65	29,91		W	1	Cloudy.
28	7	0	58	63,5	30,10		W	1	Fine.
	2	0	74,5	65	30,10		SW	1	Fine.
29	7	0	62	64,5	30,02		S	1	Fine.
	2	0	79	68	29,91		S	2	Fine.
30	7	0	62	67	29,70		S	2	Cloudy.
	2	0	69	68	29,67		SW	2	Cloudy.

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1791	Time.		Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inches.	Points.	Str.	
July 1	7	0	59	67	29,68	0,037	WSW	1	Cloudy.
	2	0	71	67,5	29,88		WSW	1	Fair.
2	7	0	54,5	66,5	29,95	0,270	S	1	Rain.
	2	0	69,5	66,5	29,88		WSW	2	Cloudy.
3	7	0	57	65,5	29,86	0,087	WSW	1	Cloudy.
	2	0	71	67	29,83		WSW	2	Cloudy.
4	7	0	59	66	29,54	0,157	WSW	2	Fair.
	2	0	62,5	66	29,55		W	2	Rain.
5	7	0	53,5	64	29,70	0,115	W	2	Fine.
	2	0	67	64,5	29,81		W	2	Fair.
6	7	0	53	62	29,90	0,081	W	2	Fair.
	2	0	66,5	63,5	29,98		W	2	Fair.
7	7	0	52	61	30,09		W	2	Fair.
	2	0	66,5	62,5	30,08		W	2	Cloudy.
8	7	0	57	62	30,05		W	1	Cloudy.
	2	0	70,5	63,5	30,01		W	1	Cloudy.
9	7	0	54,5	62	30,04		W	1	Cloudy.
	2	0	66,5	62,5	30,04		W	1	Cloudy.
10	7	0	57	62	29,90		W	2	Cloudy.
	2	0	62	62,5	29,83		WNW	2	Cloudy.
11	7	0	57	62	29,44	0,361	NE	1	Rain.
	2	0	62	62	29,53		NE	2	Cloudy.
12	7	0	54	61	29,80	0,035	NW	2	Cloudy.
	2	0	62	61,5	29,83		NW	2	Cloudy.
13	7	0	56	61	29,90		WNW	1	Cloudy.
	2	0	63	62	29,96		WNW	1	Cloudy.
14	7	0	53	61	30,08		WNW	1	Fine.
	2	0	68	62	30,11		WNW	1	Fair.
15	7	0	55	62	30,24		W	1	Fine.
	2	0	73,5	63,5	30,24		NE	1	Fine.
16	7	0	57	63	30,15		NE	1	Fine.
	2	0	70,5	65	30,06		E	1	Fine.

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1791	Time.		Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inches.	Points.	Str.	
July 17	7	0	61	64	30,01		E	1	Cloudy.
	2	0	75,5	66	30,00		E	1	Cloudy.
18	7	0	62	66	29,90		E	1	Cloudy.
	2	0	78,5	68	29,80		ESE	1	Cloudy.
19	7	0	60	66	29,77	0,030	WSW	2	Cloudy.
	2	0	72	68	29,77		WSW	1	Fair.
20	7	0	59	66	29,88		WSW	1	Fair.
	2	0	65	66	29,89		WSW	1	Cloudy.
21	7	0	58	64,5	30,00		W	1	Cloudy.
	2	0	71,5	66,5	30,04		W	1	Fair.
22	7	0	55,5	64,5	30,11		WNW	1	Fair.
	2	0	72	66,5	30,08		W	1	Fine.
23	7	0	59	64	29,90		WSW	2	Cloudy.
	2	0	71,5	66	29,88		W	2	Fair.
24	7	0	58	65	29,83		W	1	Cloudy.
	2	0	59	64,5	29,80		WSW	1	Rain.
25	7	0	54	63,5	29,70	0,301	SW	2	Cloudy.
	2	0	69,5	64,5	29,70		SW	2	Fair.
26	7	0	58	63	29,70		SW	2	Cloudy.
	2	0	67	65	29,74		SSW	2	Cloudy.
27	7	0	57	63,5	29,58	0,675	S	2	Rain.
	2	0	65	64,5	29,66		SW	2	Cloudy.
28	7	0	56	63,5	29,96	0,045	SW	1	Fair.
	2	0	68	64	29,97		WSW	2	Cloudy.
29	7	0	60	63	29,86		SW	2	Fair.
	2	0	73	65	29,80		SW	2	Fair.
30	7	0	59	64	29,85		SW	2	Cloudy.
	2	0	65	65	29,93		SW	2	Cloudy.
31	7	0	59	64	30,01		SW	1	Cloudy.
	2	0	72	65	29,95		SW	1	Fine.

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1791	Time.		Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inches.	Points.	Str.	
Aug. 1	7	0	62	65	29,93		SW	1	Cloudy.
	2	0	70	66	29,94		SW	1	Cloudy.
2	7	0	57	65	30,12	0,100	SW	1	Fair.
	2	0	68	66	30,16		WNW	1	Cloudy.
3	7	0	55	64	30,32		W	1	Fine.
	2	0	67	65	30,32		WNW	1	Fair.
4	7	0	59	64	30,30		W	1	Cloudy.
	2	0	72	65,5	30,28		W	1	Fair.
5	7	0	59	64,5	30,31		W	1	Fair.
	2	0	74	66,5	30,29		W	1	Cloudy.
6	7	0	66	66	30,16		N	1	Cloudy.
	2	0	78,5	67	30,11		N	1	Fair.
7	7	0	66	66,5	30,04		W	1	Cloudy.
	2	0	76,5	68	30,02		W	1	Fair.
8	7	0	59	67	30,08		N	1	Fair.
	2	0	73	69,5	30,08		N	1	Fair.
9	7	0	58	67	30,18		E	1	Fair.
	2	0	68	68,5	30,13		E	1	Fine.
10	7	0	60	67	29,98		E	1	Cloudy.
	2	0	70	68	29,94		E	1	Cloudy.
11	7	0	61	67	29,90		NE	1	Cloudy.
	2	0	76,5	69	29,88		N	1	Fine.
12	7	0	60	68	29,85		E	1	Fair.
	2	0	74	69	29,86		S	2	Fair.
13	7	0	63	68	30,02		W	1	Cloudy.
	2	0	73,5	69	30,09		W	1	Fair.
14	7	0	63	68,5	30,20		E	1	Fine.
	2	0	75	72	30,12		SE	1	Fine. { much thunder & light^g last night.
15	7	0	67	70	30,00	0,053	SW	1	Fine; -
	2	0	74,5	72	29,95	0,348	ESE	1	Cloudy.
16	7	0	67	70,5	29,88		WSW	1	Cloudy.
	2	0	74,5	72	29,90		WSW	1	Cloudy.

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1791	Time.		Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inches.	Points.	Str.	
Aug. 17	7	0	61	70	30,00	0,056	SSW	2	Fine.
	2	0	68	70	30,05		WNW	1	Cloudy.
18	7	0	58	68	30,32		NNW	1	Fine.
	2	0	69	69	30,32		NNW	1	Fine.
19	7	0	58	67,5	30,44		NNW	1	Fine.
	2	0	67,5	68,5	30,46		NNE	1	Fair.
20	7	0	56	66	30,52		E	1	Fine.
	2	0	69	68	30,46		E	1	Fine.
21	7	0	58	66,5	30,37		ENE	1	Cloudy.
	2	0	66	67,5	30,28		E	1	Fine.
22	7	0	59	67	30,14		E	1	Fair.
	2	0	74	68	30,08		SE	1	Cloudy.
23	7	0	57,5	67	30,00		WSW	1	Hazy.
	2	0	76,5	69	29,99		W	1	Fine.
24	7	0	64	68	29,94		W	1	Cloudy.
	2	0	67,5	69	29,94		SW	1	Cloudy.
25	7	0	60	66	29,94		W	2	Cloudy.
	2	0	71,5	68	29,91		W	2	Cloudy.
26	7	0	57	66	29,88		W	2	Fair.
	2	0	68	67	29,95		W	2	Cloudy.
27	7	0	56,5	65,5	29,81	0,160	SSW	2	Rain.
	2	0	69,5	67	29,77		W	1	Cloudy.
28	7	0	57	65	29,66	0,107	W	2	Cloudy.
	2	0	69,5	65,5	29,65		W	2	Fair.
29	7	0	57	64	29,74		WNW	1	Cloudy.
	2	0	65	65	29,85		WNW	1	Fair.
30	7	0	50	63	29,96		WNW	1	Fine.
	2	0	60	63	29,98		WNW	1	Cloudy.
31	7	0	51	62	29,99		E	1	Cloudy.
	2	0	54,5	62	29,81		SSW	1	Rain.

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1791	Time.		Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inches.	Points.	Str.	
Sept. 1	7	0	52,5	62	29,68	0,276	NW	2	Cloudy.
	2	0	64	64	30,03		WNW	1	Fair.
2	7	0	54	62	30,12		SW	2	Cloudy.
	2	0	67	63,5	30,02		SW	2	Fair.
3	7	0	61	63	29,77	0,015	SW	2	Cloudy.
	2	0	66	63,5	29,68		SW	2	Cloudy.
4	7	0	56	64	29,52	0,191	W	2	Fine.
	2	0	67	65	29,54		W	2	Fair.
5	7	0	54	64	29,96		W	1	Fair.
	2	0	66	65	30,02		SW	1	Fair.
6	7	0	62	64	29,99		SW	2	Cloudy.
	2	0	66	65	29,99		SSW	2	Cloudy.
7	7	0	65	65	29,99		SW	2	Cloudy.
	2	0	70,5	66,5	30,05		WSW	2	Cloudy.
8	7	0	60	65	30,22		NE	1	Cloudy.
	2	0	66	65,5	30,22		E	1	Cloudy.
9	7	0	63	65,5	30,15		E	1	Cloudy.
	2	0	69	68	30,05		E	1	Fine.
10	7	0	61	67	30,05		E	1	Fine.
	2	0	76	71	30,06		E	1	Fine.
11	7	0	60	69	30,17		E	1	Fine.
	2	0	77	71	30,17		E	1	Fine.
12	7	0	58	69,5	30,17		E	1	Fine.
	2	0	73	71,5	30,17		E	1	Fine.
13	7	0	52	69	30,20		NE	1	Fine.
	2	0	73	70,5	30,20		E	1	Fine.
14	7	0	55	69	30,29		NE	1	Fine.
	2	0	75	70	30,29		N	1	Fine.
15	7	0	57,5	67	30,29		W	1	Fine.
	2	0	70,5	70	30,27		N	1	Fine.
16	7	0	52	67	30,33		W	1	Hazy.
	2	0	70	69	30,21		W	1	Fine.

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1791	Time.		Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inches.	Points.	Str.	
Sept. 17	7	0	58	68	30,16		NNW	1	Cloudy.
	2	0	61,5	69	30,18		N	1	Fine.
18	7	0	53	66	29,79		W	1	Cloudy.
	2	0	60	65	29,74		WNW	1	Cloudy.
19	7	0	46	63	29,98		N	2	Fine.
	2	0	57	63,5	30,04		N	2	Fine.
20	7	0	43	61	30,10		W	1	Fine.
	2	0	60	61	30,08		WNW	1	Cloudy.
21	7	0	53	61	30,12		W	1	Cloudy.
	2	0	59	60,5	30,08		W	1	Cloudy.
22	7	0	51	60	30,10		N	1	Cloudy.
	2	0	59	62	30,08		N	1	Fine.
23	7	0	52,5	61	30,00		WNW	1	Cloudy.
	2	0	61	61	30,00		NW	1	Cloudy.
24	7	0	54	60	30,05		NNE	1	Cloudy.
	2	0	61	61	30,11		NNE	1	Cloudy.
25	7	0	49	60	30,27		NNE	1	Fine.
	2	0	62	61	30,28		NNE	1	Fair.
26	7	0	48	60	30,32		NNE	1	Fine.
	2	0	58	60	30,26		NE	1	Cloudy.
27	7	0	48	59	30,25		ENE	1	Cloudy.
	2	0	58	60	30,23		E	1	Fair.
28	7	0	50	58,5	30,25		NE	1	Fair.
	2	0	60,5	61,5	30,27		E	1	Fine.
29	7	0	46	58,5	30,31		NE	1	Fine.
	2	0	58,5	60,5	30,30		E	1	Fine.
30	7	0	48	59	30,25		NE	1	Fine.
	2	0	56	60	30,19		E	1	Fine.

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1791	Time.		Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inches.	Points.	Str.	
Oct. 1	7	0	41	58	30,04		E	1	Cloudy.
	2	0	59	59,5	29,99		W	1	Fair.
2	7	0	50	58	30,00		W	1	Fair.
	2	0	62	59,5	30,00		WNW	1	Cloudy.
3	7	0	54	58,5	30,17		NNE	1	Hazy.
	2	0	61,5	60	30,06		SSW	1	Cloudy.
4	7	0	56	59,5	29,94	0,100	ESE	1	Rain.
	2	0	57,5	60	29,87		S by E	1	Cloudy.
5	7	0	51	59,5	29,81	0,220	ENE	1	Fair.
	2	0	62,5	61,5	29,80		E	1	Fine.
6	7	0	56	60	29,80	0,012	WSW	2	Cloudy.
	2	0	61,5	60,5	29,77		SW	2	Cloudy.
7	7	0	51,5	60	29,77	0,035	W	1	Cloudy.
	2	0	58,5	60,5	29,75		W	1	Cloudy.
8	7	0	48	59	29,76		WNW	1	Fair.
	2	0	57	61	29,78		NW	1	Fair.
9	7	0	47	59	29,53	0,105	SSE	1	Rain.
	2	0	58	59	29,23		SSE	2	Rain.
10	7	0	49	58,5	29,22		S	1	Fair.
	2	0	58,5	60,5	29,27		SSW	2	Fair.
11	7	0	47	58,5	29,30	0,120	W	1	Fine.
	2	0	55	59	29,39		W	2	Fine.
12	7	0	41	57	29,74		W	1	Fair.
	2	0	54	58,5	29,79		N	1	Fine.
13	7	0	43	57	29,16		W	1	Fine.
	2	0	53	58,5	29,60		E	1	Fair.
14	7	0	49	57	29,24		E	1	Hazy.
	2	0	54	58	29,18		SSW	2	Cloudy.
15	7	0	44	56	29,38		E	1	Fine.
	2	0	53,5	58	29,34		E	1	Fine.
16	7	0	40	55,5	29,44		W	1	Hazy.
	2	0	53	56,5	29,50		W	1	Fair.

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1791	Time.		Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inches.	Points.	Str.	
Oct. 17	7	0	39,5	54	29,55		W	1	Cloudy.
	2	0	52	57	29,52		E	1	Cloudy.
18	7	0	53	57	29,27	0,161	SSW	2	Cloudy.
	2	0	56,5	58	29,41		W	2	Cloudy.
19	7	0	49	57	29,44	0,143	WSW	1	Cloudy.
	2	0	56	58	29,40		SW	1	Cloudy.
20	7	0	52	57,5	29,00	0,441	WSW	2	Cloudy.
	2	0	60	60	29,00		WSW	2	Fair.
21	7	0	54	58	28,89	0,081	SW	2	Cloudy.
	2	0	57	60,5	28,94		SW	2	Cloudy.
22	7	0	50	57,5	29,17	0,073	WNW	1	Cloudy.
	2	0	54	60	29,29		WNW	1	Cloudy.
23	7	0	39,5	57	29,66		WNW	1	Fine.
	2	0	46	60	29,76		WNW	1	Fine.
24	7	0	34	54	30,02		WNW	1	Fine.
	2	0	43	58,5	30,06		WNW	1	Fine.
25	7	0	37	54	29,92		E	1	Fine.
	2	0	49	57,5	29,77		SSE	2	Cloudy.
26	7	0	41	54	29,59	0,494	E	1	Cloudy.
	2	0	46	58	29,65		E	1	Cloudy.
27	7	0	41	54	30,02	0,042	NNE	1	Cloudy.
	2	0	48,5	58	30,15		NNE	1	Cloudy.
28	7	0	39	53,5	30,42		NNE	1	Fair.
	2	0	48,5	58	30,46		NE	1	Fair.
29	7	0	36	54	30,46		ENE	1	Fine.
	2	0	49,5	57,5	30,44		E	1	Cloudy.
30	7	0	42	54	30,30		NNE	1	Cloudy.
	2	0	47,5	55	30,22		E	2	Cloudy.
31	7	0	40	52,5	30,05		E	1	Cloudy.
	2	0	44,5	55,5	30,00		E	2	Cloudy.

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1791	Time.	Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H. M.	°	°	Inches.	Inches.	Points.	Str.	
Nov. 1	7 0	41	53	29,86		NNE	2	Cloudy.
	2 0	39	55	29,86		NE	2	Cloudy.
2	7 0	36	52	29,77		NE	1	Cloudy.
	2 0	42	53	29,67		NE	1	Cloudy.
3	7 0	36	51	29,63	0,324	NNE	1	Cloudy.
	2 0	40,5	53	29,66		ENE	1	Cloudy.
4	7 0	37	51,5	29,65	0,030	NW	1	Cloudy.
	2 0	46	53	29,64		NNE	1	Cloudy.
5	7 0	37	51,5	29,68		N	1	Cloudy.
	2 0	46	53	29,79		N	1	Cloudy.
6	7 0	32,5	50	30,13		N	1	Cloudy.
	2 0	38	51	30,18		NW	1	Fine.
7	7 0	25	48	30,22				Foggy.
	2 0	36	49	30,22		WNW	1	Foggy.
8	7 0	32,5	48	30,18		SSE	1	Fair.
	2 0	41	51,5	30,13		SW	1	Fine.
9	7 0	39	49,5	30,08		W	1	Cloudy.
	2 0	49	52,5	30,05		SW	1	Cloudy.
10	7 0	38	50,5	30,15	0,205			Foggy.
	2 0	50	53,5	30,11		S	1	Rain.
11	7 0	48,5	53	30,03	0,167	S	2	Cloudy.
	2 0	49	57	29,95		S	2	Fair.
12	7 0	42	52	29,58		SSE	2	Fine.
	2 0	48	56	29,51		S	2	Cloudy.
13	7 0	42,5	52	29,57	0,050	SSE	2	Cloudy.
	2 0	48,5	56	29,47		SSE	2	Cloudy.
14	7 0	45	53	29,05		E	1	Cloudy.
	2 0	49	56	29,03		E	1	Cloudy.
15	7 0	37	53	29,39	0,101	W	1	Fine.
	2 0	47	56	29,29		W	1	Fair.
16	7 0	42	53	28,88	0,248	WSW	2	Cloudy.
	2 0	49	56	29,03		W	2	Cloudy.

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1791	Time.		Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inches.	Points.	Str.	
Nov. 17	7	0	42	54	29,20		W	1	Cloudy.
	2	0	46	56	29,05		NE	1	Cloudy.
18	7	0	43	55,5	28,80	0,450	W	1	Rain.
	2	0	43	56	28,90		WSW	1	Cloudy.
19	7	0	46	55	28,76	0,241	E	1	Rain.
	2	0	50	58	28,89		ESE	2	Cloudy.
20	7	0	41	56	29,42	0,130	NW	1	Rain.
	2	0	48	57	29,47		NE	1	Cloudy.
21	7	0	43	55,5	29,49	0,150	NW	1	Cloudy.
	2	0	48	57	29,69		W	1	Cloudy.
22	7	0	41	55	29,94		SE	1	Fair.
	2	0	48	57,5	29,97		SSW	2	Fine.
23	7	0	47	56	30,01		S	2	Fair.
	2	0	50	58	30,01		SSE	1	Cloudy.
24	7	0	50	57	29,81	0,301	WSW	1	Cloudy.
	2	0	52,5	58	29,87		SW	1	Cloudy.
25	7	0	45	57	29,66		WSW	1	Cloudy.
	2	0	49	58,5	29,84		WSW	1	Fair.
26	7	0	45	57	30,13		W	1	Fair.
	2	0	48	59,5	30,26		W	1	Fine.
27	7	0	44	56	30,28		W	2	Fine.
	2	0	50,5	58	30,17		W	2	Cloudy.
28	7	0	52	58	29,70		W	2	Cloudy.
	2	0	52	58,5	29,53		W	1	Rain.
29	7	0	42	56	29,47	0,130	WNW	2	Fine.
	2	0	46	57	29,63		WNW	2	Fine.
30	7	0	35	54	29,78		WNW	1	Fair.
	2	0	43	56	29,57		SW	1	Cloudy.

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1791	Time.		Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inches.	Points.	Str.	
Dec. 1	8	0	38	53	29,58	0,102	WNW	1	Fine.
	2	0	44	56	29,62		W	2	Cloudy.
2	8	0	46	54	29,24		W	2	Cloudy.
	2	0	46	55,5	29,33		W	1	Cloudy.
3	8	0	37	54	29,55		WNW	1	Cloudy.
	2	0	40	55	29,51		ENE	2	Cloudy.
4	8	0	37	53,5	28,90	0,353	W	2	Cloudy; much wind
	2	0	39,5	53	29,27		NW	2	Cloudy. [last night.
5	8	0	31,5	52,5	29,75		WNW	1	Cloudy.
	2	0	34	52	29,74		SW	1	Foggy.
6	8	0	30	49,5	29,66				Foggy.
	2	0	40	50,5	29,66		SSW	1	Cloudy.
7	8	0	37	49,5	29,57		SW	1	Cloudy.
	2	0	44	51	29,43		SW	1	Cloudy.
8	8	0	34	49	29,31	0,067	WNW	1	Fine.
	2	0	38	52,5	29,38		WNW	1	Fine.
9	8	0	31,5	49	29,44		WNW	1	Fair.
	2	0	35	50	29,38		WNW	1	Cloudy.
10	8	0	31,5	48	29,31		WNW	1	Cloudy.
	2	0	35	50,5	29,35		WNW	1	Fair.
11	8	0	26	47	29,61		NW	2	Fine.
	2	0	31	48	29,68		WNW	1	Cloudy.
12	8	0	21	45	29,91		W	1	Fine.
	2	0	33	46,5	29,61		S	2	Cloudy.
13	8	0	40	46	29,11	0,045	W	1	Cloudy.
	2	0	42	49	29,10		W	1	Cloudy.
14	8	0	35	47	29,19		W	1	Cloudy.
	2	0	38	49	29,30		N	1	Cloudy.
15	8	0	33,5	47	29,65		N	1	Cloudy.
	2	0	38	48,5	29,78		NNE	1	Cloudy.
16	8	0	28	46	30,10		NNE	1	Fine.
	2	0	36	49	30,18		NNE	1	Fine.

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1791	Time.		Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inches.	Points.	Str.	
Dec. 17	8	0	29	45,5	30,38		NE	1	Fine.
	2	0	32	48,5	30,38		W	1	Fine.
18	8	0	30	46	30,23		W	1	Cloudy.
	2	0	35	47,5	30,16		WNW	1	Hazy.
19	8	0	35	47	30,00		NE	1	Cloudy.
	2	0	39	49	30,04		NE	1	Cloudy.
20	8	0	33	46	30,04		NNE	1	Cloudy.
	2	0	35	51	30,07		NNE	1	Fair.
21	8	0	33	47	30,13		NNE	1	Cloudy.
	2	0	36	51,5	30,10		NE	1	Fair.
22	8	0	30	46,5	29,96		NE	1	Cloudy.
	2	0	34	48	29,87		SSW	1	Fair.
23	8	0	43	47	29,20	0,170	SW	2	Cloudy.
	2	0	48	52	29,03		WNW	2	Cloudy.
24	8	0	33	49	29,33		WNW	2	Cloudy.
	2	0	37	49,5	29,49		WNW	2	Cloudy.
25	8	0	40	48,5	29,45	0,900	SSW	2	Cloudy.
	2	0	43	52,5	29,37		WSW	2	Cloudy.
26	8	0	34	49	29,31		WNW	1	Fine.
	2	0	40	53	29,53		WNW	1	Fine.
27	8	0	36	49	29,68		WSW	1	Cloudy.
	2	0	42	52,5	29,35		S	2	Cloudy.
28	8	0	42	50	29,29		W	2	Cloudy.
	2	0	43	54	29,58		NW	2	Fine.
29	8	0	36	50	30,03		WNW	1	Fine.
	2	0	42	54	30,15		WNW	1	Fine.
30	8	0	33	51	30,24		WNW	1	Cloudy.
	2	0	42	54	30,12		SSW	2	Cloudy.
31	8	0	46	52,5	29,72	0,297	SSW	2	Cloudy.
	2	0	47	56	29,57		SW	2	Fair.

1791.	Thermometer without.			Thermometer within.			Barometer.			Rain.*
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Inches.
January	53	31	42,1	58	46	52,9	30,58	28,18	29,56	1,957
February	52,5	30	41,1	57	48,5	52,5	30,48	29,16	29,94	0,873
March	55	31,5	44,3	61,5	48	55,6	30,67	28,90	30,20	0,716
April	64	41	51,9	64	55,5	60,0	30,11	29,08	29,77	1,460
May	67,5	39	53,1	61,5	55	58,1	30,37	29,53	30,02	0,794
June	80	47	61,3	70,5	57	63,0	30,22	29,39	29,93	0,332
July	78,5	32,5	62,6	68	61	64,2	30,24	29,44	29,89	2,194
August	78,5	50	64,9	72	62	67,1	30,52	29,65	30,06	0,824
September	77	43	59,5	71,5	58,5	62,5	30,33	29,52	30,09	0,482
October	62,5	34	48,9	61,5	52,5	57,8	30,46	28,89	29,69	2,027
November	52,5	25	43,6	59,5	48	54,5	30,28	28,76	29,68	2,527
December	48	21	36,7	56	45	50,0	30,38	28,90	29,64	1,124
Whole year			50,8			58,2			29,87	15,310

* On consulting other registers kept in and near London, it appears that the quantity of rain collected in the rain-gage of the Royal Society is remarkably deficient. Experiments are now making to determine the cause of this deficiency, and, if possible, its amount. In the mean time it was thought right to apprise the public of the fact, that no reliance may be placed on that part of the Meteorological Journal, till farther information has been obtained.

PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCXCII.

PART II.

LONDON,

SOLD BY PETER ELMSLY,
PRINTER TO THE ROYAL SOCIETY.
MDCCXCII.

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ERRATA.

PART I.

Page 52 line 9 - after water, *add* and ice.
—— 71 —— 12 - *for* bulb, *read* bottle.
—— 117 —— 6 - *dele* and.
—— 155 —— 21 and 27, *for* maggot bee, *read* bee maggot.
Meteorological journal, p. 26, l. 11, *for* 32,5, *read* 52.

PART II.

Page 314 line 27 - - *for* 0,36″ *read* 0″,36
—— 317 —— 27 - - 2,6″ — 2″,6
—— 326 —— 27 - - K — *k*
—— 347 —— 11 - - 105 — 10,5
—— 348 —— penult. - 105 — 10,5
—— 351 —— 15 - - $10\frac{3}{4}$ — $10\frac{1}{3}$
—— 355 —— 3 - - 0,94″ — 0″,94

PHILOSOPHICAL TRANSACTIONS.

IX. *On the Conversion of the Substance of a Bird into a hard fatty Matter. In a Letter from* Thomas Sneyd, *Esq. to Sir* Joseph Banks, *Bart. P. R. S.*

Read March 29, 1792.

SIR,

I TAKE the liberty of sending you two or three pieces of a bird whose substance has been converted into a hard fatty matter, which I found at the head of a fish pool, where a small brook runs into it, lying under water upon the mud. When first taken out, it was almost entire, and had several feathers sticking in different parts of the skin, which have since fallen out; a little down, however, still adheres to the smaller specimen. From the size, and general appearance of the bird, I conjectured it to be a duck, or young goose; but before I had time to give it a particular examination, it was unfortunately broken in pieces, and the greatest part destroyed. The skin in the piece which was saved is of different thicknesses, in some parts a full quarter of an inch; it has retained its original structure exactly, but is in great part separated from the flesh, though both of them are now composed of the same fat matter. This substance resembles spermaceti in its consistence between the teeth, but has neither taste nor smell; it melts in a small heat, and when congealed again, becomes more solid, and looks like wax; in a greater heat it burns, and emits a strong animal smell. As I never heard or perceived that the water in which this bird lay has any particular property, I am inclined to think that it has undergone this singular change while buried in the mud, and that the brook had afterwards washed it up, and carried it into this pool.

I am sorry that the specimen and my account of this singular metamorphosis are so imperfect. The analogy which the case bears to the change of human bodies, observed by M. Fourcroy * in the Cemetery *des Innocents*, is my chief reason for offering them to you; and if they should be deemed worthy of the notice of the illustrious Society over which you preside, you are at liberty to present them.

I am very respectfully, &c.

THO. SNEYD.

Belmont, near Leek, Staffordshire,
March 16*th*, 1792.

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* *Annales de Chimie*, Tom. V. p. 154.

X. *An Account of the remarkable Effects of a Shipwreck on the Mariners; with Experiments and Observations on the Influence of Immersion in fresh and salt Water, hot and cold, on the Powers of the living Body. By* James Currie, *of Liverpool, M. D. Fellow of the Royal College of Physicians at Edinburgh. Communicated by* Thomas Percival, *M. D. F. R. S.*

Read April 19, 1792.

THE following narrative is submitted to the Royal Society, as containing in itself some curious circumstances, and as having suggested the experiments afterwards to be recited.

On the 13th of December, 1790, an American ship was cast away on a sand-bank that lies in the opening of the river Mersey into the Irish Channel. The crew got on a part of the wreck, where they passed the night; and a signal which they made being discovered next day from Hillberry Island, a boat went off, though at a great risk, and took up the survivors. The unfortunate men had remained twenty-three hours on the wreck; and of fourteen, the original number, eleven were still alive, all of whom in the end recovered. Of the three that perished, one was the master of the vessel; another was a passenger who had been a master, but had lost or sold his ship in America; the third was the cook. The bodies of these unfortunate persons were also brought off by the men from Hillberry Island, and were afterwards interred in Saint

Nicholas church-yard, amidst a great crowd of spectators. The cook, who was a weakly man, died a few hours before the boat reached the wreck, but the two masters had been long dead, and this added to the sympathy for their loss, a curiosity to inquire into its circumstances and causes. When the following particulars came to be known, this curiosity was increased. Both the masters were strong and healthy men, and one of them a native of Scotland, in the flower of life, early inured to cold and hardships, and very vigorous both in body and mind. On the other hand, several of the survivors were by no means strong men, most of them were natives of the warm climate of Carolina, and what was singular enough, the person among the whole who seemed to have suffered least was a negro.

What is extraordinary is seldom long unaccounted for in one way or other, and the death of the two masters was said to have been owing to their having taken possession of a keg which had contained cherry-brandy, and which still contained the cherries;—these, it was reported, they had kept to themselves, and eaten in large quantities after the shipwreck; and this having produced intoxication, was supposed to have hastened their death. Some experienced seamen were satisfied with this account, which indeed seemed very rational; for though spirituous liquors may fortify the body against the effects of heat combined with moisture, and may perhaps support it for a short time under great fatigue, they are, I believe, uniformly hurtful when taken under severe and continued cold. Pleased to see a doctrine becoming popular which has been so ably supported by Dr. Aiken,[*] and others, I be-

[*] See Transactions of the Philosophical and Literary Society of Manchester, V. I.

lieved it might receive a striking confirmation from this catastrophe, into the particulars of which I determined to examine accurately. I therefore obtained access to the survivors of the crew, and from them, but more especially from Mr. AMYAT, the mate, I received the information which I required.

In repeated conversations with this intelligent young man, I learnt that Capt. SCOTT, the master of the vessel, died in about four hours after the ship struck; and that Capt. DAVISON, the passenger, died in about seven: but that the incident of their having eaten cherries infused in brandy was entirely without foundation: of this he was certain, for he saw the keg, which contained the cherries, staved, while Capt. DAVISON was endeavouring to fill it with water to make grog for the crew; the cherries fell on the wreck, and were immediately washed into the sea. Mr. AMYAT expressed his surprise at the early death of the two masters, but could not assign any cause for it. He said there was no liquor of any kind saved, nor any sort of food; that the whole crew were on an equality in all points, except that some were deeper in the water than others, but that the two masters had the advantage in this respect, for they sat on the only part of the wreck that was out of the sea, whereas the poor negro, who escaped almost unhurt, was perhaps deepest in the sea of any. He explained this in the following manner. When the ship struck they cut away her masts to prevent her from oversetting, and after this she drifted over the sand bank, into what he called a "swash" on the other side. Here she floated, and they let go their best bower anchor, but it dragged, and the vessel struck again in a few minutes on another bank. In this situation she lay some time, beating against the sand, and the sea breaking over her. In a little while Mr. AMYAT saw the tar barrels, which formed her cargo, floating towards the land, and soon after the bottom parted entirely, and was carried in the same direction. Happily for the men, the part of the wreck on which they were lashed was held by the anchor, and floated in the water, a small portion of the after part of the quarter-deck being above the surface. On this sat the two masters, generally out of the sea, but frequently overwhelmed by the surge, and at other times exposed to heavy showers of sleet and snow, and to a high and piercing wind. The temperature of the air, as nearly as can be guessed, was from 30° to 33° of FAH. and that of the sea, from trials in similar circumstances, from 38° to 40°. Immediately before the two masters was Mr. AMYAT himself. As he was sitting, and the deck sloped pretty rapidly, he was generally up to the middle in the water. The situation of the rest may be supposed; some of them were up to the shoulders. They were not at any time able to change their position, but kept their legs in pretty constant motion to counteract the cold, their arms being employed in holding by the wreck.

The master of the ship, Capt. SCOTT, a native of North-Carolina, and about forty years of age, died first. As they were in the dark, Mr. AMYAT could not see his countenance; but he was first alarmed by hearing him talk incoherently, like one in the delirium of fever. By degrees his voice dwindled into a mutter, and his hearing seemed to fail. At length he raised himself up in a sort of convulsive motion, in which he continued a few seconds, and then fell back dead on the deck. This happened about eight in the evening; four hours after the ship went aground. Soon after this, Capt. DAVISON, who was about twenty-eight, began to talk incoherently, in the same manner as the other. He struggled longer, but died in the same way, at about eleven at night. The cook died in the forenoon of the succeeding day. He was a low-spirited man, and desponded from the beginning. All the rest held out, as has been already mentioned, though sorely pinched with cold and hunger, till they were taken up about three in the afternoon. Mr. AMYAT said that his hands and feet were swelled and numb, though not absolutely senseless; he felt a tightness at the pit of his stomach, and his mouth and lips were parched; but what distressed him most was cramps in the muscles of his sides and hips, which were drawn into knots. Though immersed in the sea, they were all of them very thirsty; and though exposed to such severe cold, Mr. AMYAT himself was not drowsy, nor were any of the men drowsy, nor did sleep precede death in those that perished. These facts are curious.

Reflecting on the particulars of this melancholy story, there seemed no doubt that the death of the two masters was to be imputed to their peculiar position on the wreck. Exposed to heavy showers of sleet and snow, they might suffer from being wet with fresh, rather than salt water: they might also suffer from being exposed to the cold of the atmosphere, probably seven or eight degrees greater than that of the sea. The chilling effects of evaporation might operate against them, promoted as these must have been by the high wind; or they might receive injury from their frequent immersions in the sea, producing an *alternation* in the media surrounding. This last supposition did not, indeed, strike me at this time; the others dwelt on my mind.

Of the powers attending animation, that which seems fundamental, is the capacity of the living body of preserving the same heat in various degrees of temperature of the same medium, and, indeed, in media of very different density and pressure. If a definition of life were required, it is on this faculty that it might best be founded. It is known that some fluids, applied to the skin, vary in their effects according to their impregnation. In the same degree of temperature, pure water on the surface of the body is much more hurtful than water in which salt is dissolved. Seafaring men are universally acquainted with this, and a striking proof of the truth, as well as of the importance of the observation, may be found in the Narrative of Lieut. BLIGH. Probably the saline impregnation may stimulate the vessels of the skin in some way that counteracts the sedative or debilitating action of the cold. At any rate, it seemed not unlikely that some light might be thrown on this curious subject, by observing the effects of immersion in fresh and salt water, of equal temperature, on the animal heat. And this might also assist in accounting for the death of the unfortunate men already mentioned.

EXPERIMENT I.

I placed a large vessel, containing one hundred and seventy gallons of salt water, in the open air. The atmosphere was damp, and what is called raw. The thermometer stood at 44° in the air, and this also was the temperature of the water. The subject of my experiment was RICHARD EDWARDS, a healthy man, twenty-eight years of age, with black hair, and a ruddy complexion. The hour chosen for his immersion was four in the afternoon, about two hours after his dinner; a time appointed rather for my own convenience, than as being most proper for the purpose. His heat was 98° before undressing, his pulse 100 in the minute. He was undressed in a room where the mercury was at 56°; and afterwards stood naked before the fire till his heat and pulse were examined again, and found as before. He then walked pretty briskly through a flagged passage into an open court, where the north-east wind blew sharply upon him: he was exposed to it for a minute, and then plunged suddenly into the water up to the shoulders. The thermometer, which had been kept in a jug of warm water, at the heat of 100°, was introduced into his mouth, with the bulb under his tongue, as soon as the convulsive sobbings occasioned by the shock were over. The mercury fell rapidly, and a minute and a half after immersion it stood at 87°. He remained motionless in the water, and the mercury rose gradually; at the end of twelve minutes it stood at 93°½. While he sat in the water, it occurred to me to examine his heat when he rose out of it into the air: I had reflected on the power that must be employed to keep up his heat in a medium so dense as water, and where an inanimate body, of the same bulk, would have cooled so much more speedily than in air of the same temperature. Supposing that this heat-producing process, whatever it may be, might continue its operations some time after the extraordinary stimulus (the pressure of the water) was removed, I expected to see the mercury rise by the accumulation of his heat, on changing the medium of water for air, and therefore kept him exposed, naked, to the wind two minutes after taking him out of the bath. To my surprise, although the attendants were rubbing him dry with towels during this time, the mercury fell rapidly. He was put into a warm bed, and his heat, when examined under the tongue, was 87°, at the axilla 89°. Frictions were used, and brandy mixed with water administered; but I found on this, as on all future occasions, that the best mode of counteracting the cold, was to apply a bladder, with hot water, to the pit of the stomach (the scrobiculus cordis), a fact which I think important: this being done, his shiverings, which before were severe, soon ceased, and he became more comfortable. Three hours afterwards, however, he had not entirely recovered his former heat; but by eight at night, he was in all respects as usual.

I have been very minute in detailing the circumstances under which this experiment was made; some of the particulars which, at the time, I thought of little consequence, I found afterwards of importance. The experiment itself I determined to repeat as exactly as possible.

EXPERIMENT II.

On the next day, at the same hour, the same person was again immersed, as before. His pulse previously was 85, his heat 100°. He had been put to bed an hour before, to save the time spent in undressing. The heat of the water and of the atmosphere 44°. The wind north-east, and strong. On this occasion, as before, there was a rapid fall of the mercury; the following table will save words:

	Ther.
2 min. after immersion	89° ½
3 —— - -	90 ½
4 —— - -	92 ½
5 —— - -	94 ½
6 —— - -	95
7 —— - -	95 ¾
8 —— - -	95 ¾
9 min. after immersion	95° ¾
10 —— - -	94 ½
11 —— - -	95
12 —— - -	95
13 —— - -	95 ½
14 and 15 —— -	95

At the end of fifteen minutes he was taken out, and stood three minutes, naked, exposed to the north-east wind, at the end of which time the mercury had sunk to 88°. A draught of ale was given him, and he was put into a warm bed; in three minutes after the mercury rose to 93°. An hour after his heat was 95°.

The effects produced by this alternate exposure to water and air of the same temperature, gave a new direction to my thoughts, and determined me to inquire again into this singular phænomenon. The most obvious method would have been to have prolonged the process of alternation, and replunged the person cooled by the external air into the bath; but this was running too great a risk, unless some more sudden and certain method could be found of restoring the heat that might be lost. It was prudent, therefore, to proceed more cautiously. In the next experiment I resolved to try the methods of heating as well as cooling the body.

EXPERIMENT III.

On the following day, at the same hour, the same person was again immersed in the salt-water bath. His heat previously was 98°, his pulse 100. The temperature of the air and the atmosphere, as before, 44°. The mercury sunk rapidly to 90°.

2 minutes after -	88°
3 ——— - -	88
4 ——— - -	88 ½
5 ——— -	90 ½
6 ——— - -	92
7 ——— -	92
8 ——— - -	94
9 ——— -	94
10 minutes after -	94°½
11 ——— - -	94¾
12 ——— - -	95
13 ——— - -	96
14 ——— - -	96
15 ——— - -	96
16 ——— - -	96

He was now taken out, and stood in the wind three minutes, shivering violently. This circumstance rendered it difficult to ascertain exactly the fall of the mercury, which was, however, considerable. When examined in the room in which he undressed, it stood at 90°. He was now plunged into a fresh-water warm bath, heated to $97^{\circ}\frac{1}{2}$. What is very surprising, the mercury fell two degrees. The following table will show the progress of the return of his heat.

1 min. after immersion in the warm bath, mercury	88°
2 minutes	92
3 ———	92
4 ———	94
5 minutes after	94°
6 ———	96
7 ———	96
8 ———	96
9, 10, 11, 12, to 16,	96

If the rise of heat in the cold bath at 44°, and the warm bath at $97^{\circ}\frac{1}{2}$, be compared, the first will be found more slow; but that after being sixteen minutes in the one and in the other, the heat was the same in both cases, when taken at the mouth. It must, however, be acknowledged, that in the cold bath, the extremities were chilled and cold, while in the hot bath, the heat was equally diffused. When EDWARDS got out of the hot bath, he put on his clothes, and was remarkably alert and cheerful the whole evening. Encouraged by the safety of these experiments, I resolved to increase the time of immersion in the cold bath, and to inquire more generally into its effects on the sensations, as well as heat.

EXPERIMENT IV.

At the same hour of another day, the same person was again immersed as before, his heat previously being $97^{\circ}\frac{1}{2}$, and that of the water 42°. Wind north-east, and brisk.

1 minute after, heat	90°	12 minutes -	
2 minutes -	92	13 ——— - -	
3 ——— - -	92	14 ——— - -	94°½
4 ——— - -	92¼	15 to 24, - -	94½
5 ——— - -	92	25 ——— -	94
6 ——— - -	92½	26, 27 - -	
7 ——— - -	94	28 ——— -	94½
8, 9, 10, 11 -	94	29, 30 -	94

It will be observed, that in the above table there are blanks left in the report. At such times the thermometer was taken out of EDWARDS's mouth, to admit of his answering the questions put to him. He said, that on plunging into the water he felt an extreme cold, which he could not but think was partly owing to his being exposed, naked, to the wind before; that this cold diminished, and in a little while he felt comfortable, but that after a while the sense of coldness returned, though less than at first; diminishing again, but in a less degree. At length his sensations became pretty fixed. In this state, when the water was at rest, be should not even have known, by his feelings from the upper part of his chest to the pubes, that he was in water at all. His feet and legs were very cold; so were his hands and arms; and so also the penis and scrotum. He mentioned, likewise, that he felt a cold circle round the upper part of his body, though not constantly. On examining into this, I found it was greatest at first, and that it extended over the space which, from the undulations left in the bath by the plunge of immersion, was alternately above and under the surface of the water: when the bath settled, it was little felt; but by agitating the fluid, I could reproduce it, at any time when the cold in the extremities was not so great as to prevent its being felt. This curious particular serves to explain a circumstance much dwelt on by Mr. AMYAT, in giving an account of his sufferings on the wreck; that what he felt most severely was the cramps in the muscles of his hips and sides, parts which, from his situation on the wreck, already described, must have been alternately under and above the surge. Here I must observe, that the sea did not break over the sufferers all the time they were on the wreck. The wind moderated, as well as the waves, and for the last fifteen hours, they were not at any time overwhelmed, or at least Mr. AMYAT himself was not. The cold never abated. Being all lashed to the wreck, they never changed their positions; the bodies of those who died occupied the space where they were originally placed. Mr. AMYAT, therefore, during the whole time sat nearly up to the middle in water, but subject to the variations occasioned by the motion of the sea.

To return.—When exposed naked to the wind, the mercury, in this case, sunk as usual five or six degrees, and his shiverings were great. Desirous of restoring his heat as speedily as possible, we incautiously heated the hot bath to 104°: but after being half a minute in it, he screamed out with pain, especially in his extremities, and about his scrotum. When taken out, his shiverings almost amounted to convulsion. The bath was lowered to 88°, and he was replaced in it, and its temperature progressively, but pretty rapidly, increased to 100°. He continued, however, to shiver much, his heat remaining about 90°; but a bladder, with very hot water, being introduced under the surface of the bath, and applied close to his stomach, the good effects were instantaneous, his shiverings ceased, and his heat mounted rapidly to 98°.

All these experiments having been made on one person, I determined to repeat this last on another.

EXPERIMENT V.

R. Sutton, æt. 19, of a pale complexion, and a feebler frame, was immersed in the bath, under the circumstances of the preceding experiment. His heat was previously 96°½.

½ a minute after, heat	92°	18 minutes	93°¼
1 minute	90	19 ———	93 ½
2 minutes	88 ½	20, 21	94
3 ———	89	22 ———	92 ½
4 ———	90	23 ———	92 ¼
5 ———	92	24 ———	92 ¼
6 ———	92 ¼	25 ———	94
7 to 10	92	26 ———	94
11 ———		27 ———	92 ½
12 to 15	92	28 ———	92 ¾
16 ———	92 ½	29 ———	94
17 ———	93	30 ———	94

Though this person seemed to bear the cold bath well, having lost in 30 minutes only 2½ degrees of heat, yet when exposed afterwards to the wind, he shivered violently, and lost his heat very fast. He was put into a warm bath, heated to 96°, but recovered his heat very slowly, as the following table will show.

1 minute after, heat	88°	
2 minutes	90	
3 ———	90½	
4 ———	90	great shivering.
5 ———	90	here the bath was heated to 100°.

6 ———	-	90°	shiverings still.
7 ———	-	90	ditto.
8, 9 -	-	90½	ditto.
10 ———	-	92	ditto.
11 ———	-	92	bath heated to 104°.
12 ———	-	94	
13 ———	-	93	——— heated to 108°. Shiverings.
14 ———	-	93	a bladder with very hot water applied to the stomach.
15 ———	-	94	
16 ———	-	96	very comfortable.

EXPERIMENT VI.

RICHARD EDWARDS, the original subject of experiment, was again immersed in the cold bath, of the temperature of 40°, and remained in it three quarters of an hour. His heat previously was 97°; his pulse 90 in the minute. The mercury fell to 92°, was stationary for a few minutes, and then mounted, though, as usual, with no regularity. In twenty-two minutes it stood at 96°; it then began to decline, and in twenty-three minutes more had sunk to 94°. Being exposed as usual to the wind, the mercury sunk as usual, and he shivered violently. In the warm bath at 96° his shiverings continued several minutes, his heat remaining at 90 and 91°. In seven minutes the mercury began to rise fast, and five minutes after was at 96°.

EXPERIMENT VII.

The effects of forty-five minutes immersion in the cold salt-water bath, at 40°, were proposed to be tried on RICHARD SUTTON. He was much under the impressions of fear, and his heat previously raised the mercury only to 94°. The mercury sunk, as usual, on his immersion, but to an unusual degree. It did not stop in its fall till it got to 83°, which perhaps might be in part accounted for by the extraordinary chattering of his teeth, admitting some contact of the air. It then mounted in the usual irregular way, and at the end of thirteen minutes had got to 92°. Here it stood for nineteen minutes longer with little variation; at the end of this time it began to fall rapidly, though irregularly, and in three minutes was down at 85°. He had now been thirty-five minutes in the water, and I did not think it safe to detain him longer; we therefore hurried him into a warm bath, heated to 96°, where he shivered much. The bath was heated gradually to 109°, and in this heat he recovered his proper temperature in about twenty-eight minutes. Being then put into a warm bed, he fell into a profuse perspiration, which left him in his usual health.

One general remark will serve for the pulse in all these experiments. It was not possible to keep the subjects of them from some degree of previous agitation, and this always quickened the pulse. The natural pulse of EDWARDS was about 70 in the minute; but it may be observed, that it was never slower than 85 before immersion, and generally more. However this might be, it invariably sunk to 65, or from that to 68, in the water, became firm, regular, and small. After being long in the bath, it could hardly be felt at the wrist, but the heart pulsated with great steadiness and due force. In the last experiment, when the heat sunk rapidly, SUTTON said that he felt a coldness and faintness at his stomach, which he had not perceived before, and when I felt the motion of his heart, it was feeble and languid. In some future trials of the effects of immersion in fresh water (one of which I shall detail), the same coldness at the stomach preceded a rapid fall of the mercury; and these facts, together with the effects I found from applying a considerable heat to this part when the body was chilled with cold, convince me that there is some peculiar connection of the stomach, or of the diaphragm, or both, with the process of animal heat. Whoever will consider the rapidity with which a dead body would have cooled immersed in water of the temperature of 40°, may form some estimate of the force with which the process of animal heat must have acted in the experiments already recited. These experiments, however, furnish irrefragable proofs of the futility of some of the theories of animal heat. The increase of heat, in fever, has led some persons to believe that animal heat is produced by, or immediately connected with, the action of the heart and arteries; here, however, it may be observed, that while heat must have been generated in the bath with more than fourfold its usual rapidity, the vibrations of the arterial system were unusually slow. Another, and a very beautiful theory of animal heat, supposes it immediately to depend on respiration; but in the bath, after the first irregular action of the diaphragm from the shock of immersion was over, the breathing became regular, and unusually slow. Lastly, the curious phænomenon of the heat rising, and falling, and rising again, in the bath, with the body at rest, and the temperature of the surrounding medium unchanged, is, I think, fatal to those theories of animation which consider the living body as a mere machine, acted on by external powers, but not itself originating action, and differing from other machines only in the peculiarity of the powers which are fitted to set it in motion. I have said that the temperature of the medium continued unchanged, but it may be supposed that the bath was heated a little during the experiments; it was so; but being exposed, with a large surface, to the open air, the wind blowing briskly over it, its heat was little altered; in twelve minutes immersion it had gained nearly one degree, and in forty-five minutes, the longest duration of any of the experiments, it had gained three degrees. As this accession was regular, if it had been greater it would not have invalidated the foregoing observations.

Many other trials were made on the effects of immersion in water on the human heat, which I shall speak of generally, under the general conclusions which they suggested.

The experiments already recited, suggested to me the notion that in all changes from one medium to another of different density, though of the same temperature, there is a loss of animal heat. I found, however, that this conclusion requires many restrictions.

1. My experiments being made on bodies of such very different density as air and water, do not admit an universal inference of this sort.

2. Being all made in a temperature fifty degrees under the human heat, no certain conclusion can be drawn as to what might happen in degrees of heat much higher, where it is probable, the effects of the change, if it appeared at all, might be

less striking. It would seem, however, that after a person is long chilled in cold water, the first effect of passing through the external air into the warm bath, is first a fall of heat in the air, and after this a still greater fall in the warm bath, followed, however, by a speedy rise.

The air and the water being equally cold, and both 45° or under, I found the loss of heat in passing from the one to the other to be regulated in the following way.

1. If, instead of being exposed naked to the wind previous to immersion in the water, the body was kept warm by a flannel covering, the mercury fell much less on the first plunge.

2. If, after plunging into the water, the person continued in it only a minute or two, a subsequent fall of the mercury did not always take place, on his emerging into the air. On the contrary there was sometimes a rise on such occasions in the mercury, especially if the atmosphere was at rest.

3. In one instance, after continuing in the water fifteen minutes, on rising into the air in a perfect calm, though during a frost, there was little or no seeming diminution of the heat; while exposure under similar circumstances, with a north-east wind blowing sharply, though the air was many degrees warmer, produced a rapid diminution. The effects of the wind in diminishing the human heat are indeed striking, and are not in my opinion explained by the common suppositions.

4. The loss of heat by a change of media, depends much on the rapidity of the change, for the plastic power of *life* in varying the process of animal heat, so as to accommodate it to the external changes, acts for a time with great celerity, though this celerity seems to diminish with the strength.

EXPERIMENT VIII.

I placed in a large room, where the mercury stood at 36° two slipper baths at the distance of six yards from each other. One was filled with cold salt-water of the temperature of 36°, the other with water heated to 96°, which was my own heat. Undressing myself in an adjoining room by a fire, I afterwards slipped on a loose flannel dress, and descended *slowly* into the cold bath, where I remained two minutes; I ascended *slowly* into the air, and then sunk myself in the warm bath, where I remained two minutes also: I returned to the cold bath, where I staid two minutes as before, and removed from it again to the warm bath. But during all these changes of media and temperature, the thermometer with its bulb under my tongue never varied from 96°. I attribute this partly to the heat of my body being in some degree defended by the flannel dress, partly to the calm of the air, but chiefly to the slowness of motion in these changes. It may be said that the time of staying in the different baths was not long enough to produce any sensible change in the heat of circulating fluids of such a mass, but this is not consistent with many of the other facts.

5. The influence of the application of cold water to the surface of the body on the heat, is in some respects regulated by the animal vigour, as the following experiment will show.

EXPERIMENT IX.

In the same room I placed a large empty vessel: in this two young men sat down in succession, each with the bulb of a thermometer under his tongue. A man standing on a bench with a bucket of cold salt-water containing four gallons, poured the whole on the head and shoulders, suffering it to run down on the rest of the body. This process took up nearly a minute, during which I examined the mercury, and found it unchanged. They were both directed to continue sitting without motion for a minute after, during which, in both instances, the mercury rose two degrees. A third, much inferior in vigour, submitted to the same experiment, and the mercury continued during the affusion of the water unchanged, but in a minute after sunk half a degree. In fevers, where the heat is generally increased from two to six degrees above the standard of health, pouring a bucket of cold water on the head always reduces the pulse in frequency, and commonly lowers the heat from two to four or five degrees. Of this salutary practice I hope soon to speak at large to the public.

6. The power of the body in preserving its heat under the impressions of cold, and the changes of temperature, and of media, seems in some measure regulated by the condition of the mind. That fear increases the influence of cold, and of many other noxious powers, will not be doubted ; but the state of the mind to which I allude, is that of *vigorous attention* to other objects. This, it is well known, will to a certain degree deaden, or, indeed, prevent the sensation of cold ; and what does this, I apprehend, prevents, or at least weakens, its physical action. The astronomer, intent on the objects of his sublime science, it is said, neither feels, nor is injured by, the damps nor the chillness of the night ; and in some species of madness, where the ideas of imagination are too vivid to admit the impressions of sense, cold is resisted to an extraordinary degree. I have seen a young woman, once of the greatest

delicacy of frame, struck with madness, lie all night on a cold floor, with hardly the covering that decency requires, when the water was frozen on the table by her, and the milk that she was to feed on was a mass of ice.

7th. There are particular conditions of the atmosphere, not perfectly understood, that seem to have an influence in depriving us more speedily of our animal heat, than others where the cold is greater.

It may seem that by this time I had renounced my intention of trying the effects of immersion in fresh water on the animal powers, and particularly on the heat. Some trials I have, however, made, of which I shall only relate the following.

EXPERIMENT X.

In the same vessel, containing an equal bulk of fresh water, Richard Edwards, the subject of my first experiments, was immersed, at the same hour of the day. His heat previously was 98°, his pulse beat 92 in the minute: the heat of the air was 41°½, that of the water 40°. The wind was now in the west, so that in the court where the bath stood there was a perfect calm. As I had some fears of the issue of this experiment, instead of exposing him for a minute naked to the wind before immersion, he was covered with a flannel dress from the air till the instant he descended into the water, into which he was suffered to sink himself slowly, with the bulb of the thermometer under his tongue. These are important circumstances. The following table exhibits the result.

Immediately on immersion, heat	98°
1 minute after	97½
2 minutes	97
3 ———	98
4 ———	97½
5 ———	96
6 ———	96
7, 8	96
9 ———	97
10 ———	97
11, 12, 13	
14 min. after, heat	96°½
15	96
16, 17, 18, 19, 20	96
21, 22, 23, 24	
25 ———	95
26 ———	94
27 ———	93½
28, 29	94
30	93
31, 32	94
33, 34	92½

He now got out into the air very slowly, and stood in it three minutes, the wind *not* blowing on him. He lost one degree of heat at first, which he recovered. He was then put into a warm bath at 90°, which at first *he felt* warm, and his feet and hands were pained: but in two minutes he fell into a very violent shiver, and his heat fell two degrees. The bath was then heated to 95 and 96°, but still he felt cold. It was heated to 99°: he continued in it five minutes, and his heat was 91°. The heat was gradually raised to 106°, when the sense of coldness of which he had complained at the pit of the stomach gradually went off. Before this I had usually kept him in the warm bath till his natural heat was nearly recovered, but after being half an hour in the heat of 106°, his own heat was still 93°. He now became sick and very languid, a cold sweat covering his face, his pulse very quick and feeble. He was removed into bed, but passed a feverish night, and next day had wandering pains over his body, with great debility, resembling the beginning stage of a fever. By cordials and rest this went off.

This experiment clearly enough confirms the greater danger of being wet with fresh than salt water; but in itself points out nothing certain besides, except that it is not to be rashly repeated. I mean to try some of these experiments to a greater extent on the brute creation, when I have procured thermometers better suited to my views. The thermometers I employed had not a sufficient mobility for very nice experiments, and I am well aware that in particular instances this may have misled me, though the general results, which is all that is of importance in such experiments as these, will, I hope, be found just and true.

Before I conclude, I must offer a few observations on the subject that led to these experiments.

1. It is, I think, already well known among seamen, that where there is only the choice of being wet with salt or fresh water, it is always safest to prefer the first. In the heavy showers of rain, hail, or snow, by which gales of wind are generally accompanied, the men that must be exposed to them, ought, like Lieutenant BLIGH and his crew, to wring their clothes out of salt-water.

2. In all cases where men are reduced to such distress by shipwreck or otherwise, that they can only chuse between the alternative of keeping the limbs constantly immerged in the sea, or of exposing them to the air while it rains or snows, or the sea is at times washing over them, it is safest to prefer a constant immersion; because, in the northern regions where the cold becomes dangerous to life, the sea is almost always warmer than the air, as the experiments of Sir CHARLES DOUGLAS shew; and because there is not only a danger from the increased cold produced by evaporation, but also from the loss

of heat by the rapid changes of the surrounding medium, as the foregoing experiments point out.

3. Whether, in high and cold winds without rain or snow, and where a situation may be chosen beyond the reach of the waves, it is safer to continue in the air, or to seek refuge in the sea, must depend upon several circumstances, and cannot perhaps be certainly determined. The motives for choosing the sea will be stronger in proportion as the wind is high and cold, and in proportion as the shore is bold.

The foregoing narrative shows that men may survive twenty-three hours immersion in the sea, of the temperature of 38° or 40° (as great a cold as it almost ever possesses) without food or water, and almost without hope of relief; but that any man ever survived an equally long exposure to the higher degrees of cold of the atmosphere, in the same circumstances, does not appear. Though in the case related, immersion in water did not prevent thirst, yet there is no doubt that it alleviated it, a circumstance of high importance towards the preservation of life.

Liverpool,
December 25, 1791.

P. S. I have purposely avoided any reasoning on the causes of the loss of vital heat on the change of media in the experiments recited. It may be supposed that during immersion, the water immediately in contact with the skin having become heated to a certain degree, the naked body, on rising from it into the air, was in fact exposed to a colder medium, and thus the loss of heat, in this instance, produced. My examination of the heat of the water during immersion not having been made in contact with the body, I will not deny that there is some foundation for the remark; and the cases, it must be allowed, are by no means exactly parallel between immersion in an open vessel, however large, and immersion in the sea, where the constant undulation may be presumed to occasion a continual change in the surrounding fluid. But whatever allowance may be made for the circumstance mentioned, I am persuaded that the difference between the density of air and water being considered, it is not sufficient to explain the loss of heat in the instance alluded to. The changes of temperature in the living body are governed by laws peculiar to itself. I have found, in certain diseases, greater and suddener variations than any mentioned, from applications of cold very gentle in degree, and momentary in duration.

In his masterly "Experiments and Observations on Animals producing Heat," Mr. HUNTER has objected to taking the heat of the human body by introducing the bulb of the thermometer into the mouth, because it may be affected by the cold air in breathing. The objection is well founded if the bulb be placed on the upper surface of the tongue, but if it be under it and the lips shut, the effects of respiration may be disregarded, as I have found from many hundred experiments. The heat may be observed in this way with ease and certainty, by employing thermometers curved at that end to which the bulb is affixed (the bulb being introduced at the corner of the mouth), some of which have been made for me by Mr. RAMSDEN according to a form given, as well as others on Mr. HUNTER's plan. From repeated trials it appears to me, that when the usual clothing is on, the heat of the living body may be taken, with nearly the same result and equal certainty, under the tongue with the lips shut, at the axilla with the arm close to the side, and in the hollow between the scrotum and the thigh; every other part of the surface is liable to variation and uncertainty. It is evident that of these three methods, the first only can be employed (as far as I can discover) when the trunk of the body is immersed in water; and even when the naked body is exposed to the cold air, the first method seems the best, the heat remaining most steady under the tongue: the axilla is the next best in order, and the worst, the lower part of the groin; for the scrotum and the parts of generation lose their heat on the application of cold more speedily perhaps than any other part of the body, the extremities not excepted.

N. B. The water employed in the experiments related, contained salt in the proportion of one to twenty-four.

Instead of saying that the men saved were most of them natives of Carolina, I find I ought to have said, men long accustomed to that country and other warm climates, but not most of them natives.

XI. *A Meteorological Journal, principally relating to Atmospherical Electricity; kept at* Knightsbridge, *from the 9th of* May, 1790, *to the 8th of* May, 1791. *By Mr.* John Read; *communicated by* Richard Henry Alex. Bennet, *Esq. F. R. S.*

Read April 26, 1792.

A DESCRIPTION of the instruments for collecting atmospherical electricity, used in the following journal.

Fig. 1 and 2 (Tab. IV.) represent the apparatus. AA fig. 1, is a round deal rod, 20 feet long, two inches diameter at the lower, and one inch at the upper end: into the lower end of it is cemented a solid glass pillar B, 22 inches long; the lower end of this glass stands in a socket of wood C, which is screwed on the garret floor D, and supports the whole. About seven feet above the floor, is firmly fixed to the wall a strong arm of wood E, which holds perpendicularly a strong glass tube F, through which the rod is slided gently upwards, till the glass pillar B may be lowered into the socket C. It is thus fixed, and stands 12 inches from the wall. The tube F is of sufficient width to admit a case of cork, which is fastened in the inside of it, at the part where the tube is sustained by the arm of wood E, so that the rod, when bent by the wind, cannot touch the tube or break it. The upper extremity of the rod is terminated by several sharp-pointed wires G; two of them are of copper, each one-eighth of an inch thick; and, in order to stiffen the rod, as well as conduct more readily the electric fluid, one of them is twisted round the rod to the right hand, and the other to the left, as low down as the brass collar H, to which they are soldered, in order to render their contact perfect. A little above the upper end of the glass pillar B, is placed a brace II of solid glass sticks, to keep steady that part of the rod. K is a hollow cylinder of wood, 12 inches within, proceeding from the ceiling through the roof LL, above which there is a hollow tin cover M, 24 inches within, fixed to the rod; which serves to defend the open cylinder K from the weather. At a convenient distance from the foot of the rod is a hole bored through the floor and ceiling. This hole receives a glass tube covered with sealing-wax (as are all the other glasses in this apparatus), through which a strong brass wire proceeding from the rod is conveyed into the room below, wherein, at a convenient height from the floor, it terminates with a two-inch brass ball N. A strong ring of brass is made to move easy on the wire, and rests on the brass ball; into this ring is screwed a brass wire seven inches long, which keeps suspended at its extremity a pith-ball electrometer O.

At two inches distance of the above-mentioned brass ball N, a bell P is supported by a strong wire, which passing through a hole made in the wall, is made to communicate, by means of a good metallic continuation Q, with the moist ground adjoining to the house. A brass ball, three-tenths of an inch in diameter, is suspended between the bell P and ball N, by a silk thread fastened to a nail R. This ball serves for a clapper, by striking between the ball and bell, when the electrical charge of the rod is sufficiently strong. V gives nearly a front view of this part of the apparatus.

S is a small table fixed to the wall under the bell and ball, at a convenient height above the floor, upon which Leyden bottles and other apparatus are occasionally placed. Any person versed in the science of electricity, will easily understand that this apparatus is calculated to show the various degrees of intensity of atmospherical electricity ; and at the same time to avoid the pernicious effects which may be occasioned by thunder-storms, or in short by any great quantity of electricity in the atmosphere.

The whole perpendicular height of both parts of this apparatus taken together, from the moist earth to the point at the top of the rod, is 61 feet. If the insulation could be constantly kept in due temperature, with respect to heat and cold, I imagine it would always be electrified. But I fear that cannot be done, without the aid of common fire ; which in so large an apparatus would be very difficult, so that there might not be too much nor too little of it. I am inclined to this opinion, from the success I have had in a number of experiments, in which the aid of common fire was applied to improve the insulation, as well as to collect the electric fluid.

When I find that the moisture in the air has so much injured the insulation of my high pointed rod, that it will not retain a weak electricity, in that case I make use of my hand exploring rod, which is about the length and thickness of a common fishing-rod, with plenty of small wire twined round it from end to end. The method of using it is simple and easy. A representation of it may be distinctly seen in fig. 2. Having first *warmed* the glass legs of the stool, I place myself upon it, and raise the rod into a vertical position, keeping it so for a minute or two; I then with a finger of the other hand touch a sensible electrometer, and if the threads open, it is sufficient. But should the electrical state of the atmosphere be too weak to produce that effect, which seldom happens, then in that case I add to the rod a *lighted torch* T, and place it as remote from my hand as the strength of the rod will bear, and repeat the experiment; thus circumstanced, it has never yet failed me.

It will be necessary just to mention the method I have pursued in forming the journal of atmospherical electricity. This has been principally by means of the signs exhibited by the pith balls O, connected with the rod. When I find these closed, and attracted on the approach of my finger, yet not sufficiently charged to repel each other, I write weak signs of electricity. When I find the balls open, and, on the approach of excited glass, the balls close, I write they are electrified positively; but, if the balls open wider, I write they are electrified negatively; and the reverse when I use sealing-wax. When the balls diverge one inch and upwards, visible sparks may be drawn at the brass ball N. When sparks are said to have been perceived in any observation, I have generally on that account omitted to note the variable quantities of divergency in the pith balls. Their utmost limit of regular divergency seems to be about five or near six inches; above that they are unsteady and disorderly. The pith balls are near two-tenths of an inch in diameter, suspended by very fine flaxen threads (in the state it is in from the heckle) five inches long. When I mention the distance of the balls in tenths of an inch, it is to be understood as nearly so as my eye can determine.

This apparatus requires a constant attention, especially during a disturbed state of the atmosphere. From the room in which the apparatus is placed I am seldom absent one hour, excepting the time of sleep ; but, when I leave it, the last thing I do at night is to examine the state of the electricity, and, if I find the rod unelectrified, I then place the Leyden bottle on the table S, with its knob nearly in contact with the ball N. The next morning, if I find this bottle charged, I write the kind of electricity it is charged with against the day in the journal, and add, *by the night bottle*.

It is presumed, that the table is sufficiently obvious. The two columns for positive and negative electricity are used only for the first observation of each day. I use FAHRENHEIT's thermometer, suspended on the north outside of a bow-window. The time of making the observation with it, and the barometer, and also of the direction of the wind, has usually been nine o'clock in the morning.

Lastly, it may be useful to observe, that I have always found, when the rod is highly electrified, the lower though *uninsulated* part of the apparatus *(viz.* the metallic connection of the bell P with the moist earth) to be in a contrary state of electricity to the upper and insulated part. See the 20th of May.

Having made a memorandum of the several thunder-storms which have happened in divers parts of this island, according to information by letters, and from newspapers, I thought it useful to insert them in this journal, in order to show whether some contemporaneous appearances in my apparatus might not be attributed to them.

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	May 9, 1790.
		Inches.	°				
May 9	N	29.14	67	strong	pos.	—	A clear fine morning. One o'clock P.M. a sudden shower of rain fell; the pith balls O, connected with the lower end of the rod, were now charged negatively, and increased in strength as the rain came on; in a short time the electricity changed to positive. Five o'clock, the electricity changed again to negative, I then heard some distant rumbling of thunder. This day there was some lightning at Hertford.
10	NE	29.18	66	0.	—	neg.	A.M. sky very cloudy, and some small rain falls.
11	E	30. 5	64	0	pos.	—	Strong gales of wind; and a weak electricity.
12	E	29.14	70	0	pos.	—	
13	E	29. 6	72	0	pos.	—	
14	NE	30. 9	72	0	pos.	—	
15	N	29. 5	65	0	pos.	—	A.M. weather serene.
16	NE	29. 2	57	0	pos.	—	A.M. a clear sky. Some lightning at Bishop-Stortford.
17	S	29. 8	58	strong	pos.	—	Half after nine o'clock A.M. fell a little rain; as it increased so did the electric charge in the rod, insomuch that it streamed forth from the brass ball N for a short time.
18	SW	29.90	59	small	pos.	—	Six o'clock A.M. One o'clock P.M. a low black sky, by which the charge in the rod became negative, with sparks; after continuing so one hour, the electricity changed again to positive. The rod has been in charge 16 hours this day; how much more I cannot say.
19	S	29.20	65	strong	—	neg.	A.M. At four o'clock P.M. fell a mild rain, void of the electric fluid. At five o'clock the rain fell more copiously, and it was strongly charged with negative electricity.
20	S	29.14	64	small	—	neg.	Early this morning I found the humidity of the air electrified negatively. At ten o'clock, the electricity became more intense, so as to emit small sparks: in half an hour's time there were four beautiful gradual changes of electricity. Afterwards the rod remained negative two hours. Three o'clock P.M. the electricity changed, and continued near three hours positive. After this a low black cloud passed over the rod, and let fall a few drops of rain mixed with

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				
							hail; the rod was now highly charged positively, the bell rang briskly, but soon stopped, while the electricity changed to a strong negative; then it began to ring again, for about ten minutes, after which all signs of electricity ceased in the rod for a few minutes. Afterwards the rod became rather strongly charged for two hours, and changed once in that time. During this high charge of the rod, the bell (which is connected by metal with the moist ground) was in a contrary state of electricity to that of the insulated part of the apparatus; and also the air in the room, the window-shutter nearest the bell, and even the bricks in the wall, were all electrified sufficiently to indicate the kind.
May 21	SW	29.98	58	strong	pos.	—	A.M. by the night bottle, which had been charged while I slept. Four o'clock P.M. a low cloud approached the rod, and dropped a little rain, by which it became electrified strongly negative. This electrification of the rod lasted near four hours, and changed three times in kind.
22	E	29.10	56	0	pos.	—	A.M. One o'clock P.M. wind S, a small rain fell, and the air being very moist, has lessened considerably the insulation of the rod.
23	S	29.90	56	0	pos.	—	The air is dry to-day, but the clouds are very high.
24	NE	29.10	69	strong	—	neg.	Six o'clock A.M. Nine o'clock some rain fell, which increased the negative charge in the rod. The rain now falls more copiously, and the balls close and open gradually with positive electricity, which was rather strong for a short time. The balls again gradually close and open, without change of kind. The rod has been electrified near 14 hours this day. In the morning of to-day, at Lewes in Sussex, much lightning and thunder.
25	NE	29. 4	70	0	—	neg.	A.M. A moist air, and wet morning.
26	E	29.95	62	strong	pos.	—	A dark cloudy morning. Three o'clock P.M. on the approach of a heavy dark cloud, a few drops of rain fell, by which

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				
							the rod was electrified positively; the pith balls soon opened full four inches, and by and by the bell began to ring slowly. Two gentlemen fortunately being present, and much delighted with the sight, by my desire joined hands with me to make the circuit between the bell and brass ball N, and on taking a spark at the brass ball, we all received a smart shock through our arms and breasts. The wind is now S. with a heavy fall of rain; the bell (as usual) now ceases to ring while the electricity changes to negative: then rings again briskly. The electricity gradually changed twice after this, and ended negative. These several phænómena took up one hour.
May 27	N	29.98	69	0	—	neg.	A.M. A small rain. In the afternoon positive. Near midnight London was visited by vivid darts of lightning, and repeated explosions of thunder, extremely awful and terrific at that time of night. I was in bed, and awaked out of sleep by it.
28	NE	30.	61	small	—	neg.	By the night bottle. A heavy thick air. Seven o'clock P.M. positive with small sparks. Nine o'clock, fell a heavy shower of rain; its electricity was negative.
29	SE	30.	69	0	pos.	—	A.M. and P.M.
30	SW	30. 6	63	strong	pos.	—	At seven o'clock A.M. the weather showery, and the electricity rather strong, but often changing in kind. Seven o'clock P.M. one uniform black cloud darkens the air (yet the sun shines in the west); by and by a heavy rain fell, by which the rod became very strongly electrified positively, the bell rings briskly, and the pith balls are agitated with sudden jerks; the rain abates, and the electricity becomes negative. These operations of nature lasted only 34 minutes; during that time, the electricity was four times positive, and five times negative; and these several changes were all gradual.
31	N	30.	58	strong	—	neg.	The weather, and the electricity of the atmosphere have been this day nearly the same as on the preceding one.

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				
June 1	S	30.12	58	0	pos.	—	Serene fine weather.
2	N	30.16	69	0	pos.	—	
3	SW	30.12	66	0	pos.	—	
4	N	30. 8	61	0	pos.	—	
5	N	30.18	58	0	pos.	—	
6	N	30.16	62	0	pos.	—	A.M. Two o'clock P.M. the sky still clear; the electricity of the rod was now negative, and continued so two hours. During this time, the rod was only electrified with its own electricity, or what has been termed influentially electrified; for I found by trying both extremities of the rod, that they were in contrary states of electricity.
7	W	30.	62	0	pos.	—	A.M. A warm moist air.
8	SW	29.91	66	0	pos.	—	A.M. Some mizzling rain.
9	S	29.65	64	0	pos.	—	A.M.
10	SW	26.60	65	strong	pos.	—	A cloudy morning. Half after one o'clock P.M. the electric charge in the rod became negative. In a short time after, some rain fell, and the electricity again became positive, and very strong; the bell now rang slowly, and soon stopped; the pith balls closed and opened negative, and continued so a full hour: the balls exhibited one sudden jerk during that time. This charge of the rod lasted two hours. During the passage of a cloud over the rod, in the morning of to-day, I found it influentially electrified.
11	N	29.90	65	0	pos.	—	The weather serene and clear. The electricity varying between nothing and the balls seven-tenths of an inch open.
12	N	30.	59	0	pos.	—	
13	NE	30.29	60	0	pos.	—	
14	NE	30.30	64	0	pos.	—	
15	NE	30.30	69	0	pos.	—	
16	N	30.23	54	0	pos.	—	
17	E	30.10	61	strong	—	neg.	After a course of clear serene weather, this day has been dark, and hazy. Nine o'clock P.M. wind S, a prodigious large cloud approached the rod, and a little rain fell, yet there were no signs of electricity in the rod. In a few minutes after this, I was surprised by hearing the bell ring; I ran to the apparatus, and found it highly charged negatively. When the main bulk of the cloud arrived over the rod, its electricity changed to a strong positive;

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	o				
							and in a quarter of an hour, the rain and electricity all ceased together. This day, in the evening, at Wolverhampton, fell a heavy rain, accompanied with lightning and thunder.
June 18	NW	29.91	63	o	pos.	—	A.M. and P.M.
19	S	30.	68	o	—	neg.	A dark cloudy morning. The electricity positive at five o'clock P.M.
20	SW	30.21	74	o	pos.	—	Weather very hot, and a weak electricity.
21	SW	30.35	76	o	pos.	—	
22	E	30.20	79	o	pos.	—	A.M. Serene hot weather. Near sunset the wind W, when heavy black clouds were forming, their upper border about 25 degrees above the horizon. The lightning flashed among these clouds from nine to eleven o'clock. But there being neither clouds nor low vapour near the rod, it was not affected thereby. In the evening of this day at Salisbury, a very severe storm of hail, rain, lightning, and thunder.
23	SW	30.	78	o	pos.	—	A.M.
24	W	30.	68	o	pos.	—	A.M. The sky has been overcast all day, and two or three times fell a little rain, driven by a strong wind, but nearly void of electricity.
25	W	29.90	68	strong	pos.	—	At six o'clock A.M. Near eleven o'clock, a shower of rain fell, strongly negative. At the same time, in a *lower stratum* of air, the hand exploring rod was electrified *positively*. Abundance of small white clouds passed over the rod, which kept the pith balls continually closing and opening, with negative electricity. Eight o'clock P.M. some heavy drops of rain fell; the rod now became very strongly electrified; the bell rang slowly, and soon stopped, while the electricity changed to a strong positive, then rang again more briskly for a longer time. After this there were two more gradual changes.
26	NW	29.90	63	strong	pos.	—	The weather, and atmospheric electricity, have been this day nearly the same as on the preceding one; only the operations in the rod have been more powerful. For in one charge of the rod today, I counted about forty dense sparks, which spontaneously struck between

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				
							the bell and brass ball N in quick succession. These sparks were about two inches long.
June 27	N	29.96	61	strong	pos.	—	A.M. Four o'clock P.M. I saw heavy clouds forming; and as they came nearer and nearer, the rod became more powerfully electrified with negative electricity. It now began to rain, by which the rod became intensely electrified indeed. Many dense sparks now struck through the air, between the bell and brass ball N, with a loud report for so small an opening of two inches only. There were six changes of electricity in two hours.
28	W	30. 5	57	0	pos.	—	Mild foggy weather; moderately electrified.
29	N	30.	70	0	pos.	—	
30	SW	30.	67	0	pos.	—	
July 1	W	29.83	66	small	pos.	—	A.M. Negative P.M. with small sparks.
2	W	29.77	63	strong	pos.	—	A.M. Half after twelve o'clock P.M. a sudden gust of wind sprung up, followed by a shower of rain, which electrified the rod positively, and sufficient to emit small sparks; which soon afterwards became negative. Near four o'clock P.M. a very large black cloud passed over the rod, and let fall a little rain mixed with hail, by which the rod became highly electrified negatively: the bell now rang briskly, till a flash of lightning and instant crack of thunder happened, which occasioned a sudden change in the electricity to positive: then the bell struck up again as brisk as before. The rod has been charged ten hours without intermission this day.
3	NW	29.72	70	0	pos.	—	A.M. A cloudy atmosphere, but fair.
4	SW	29.66	69	0	pos.	—	A.M. Dull heavy weather.
5	SW	29.40	64	strong	—	neg.	This day has been showery and sunshine by turns. The rod has been rather strongly electrified, and there have been four gradual changes of electricity. The bell rang briskly a long time. This charge lasted full five hours.
6	N	29.70	65	0	—	neg.	A strong gale of wind with rain, and a weak electricity.
7	N	30.10	63	0	—	neg.	
8	SW	30.	63	0	pos.	—	A.M.
9	SW	29.86	67	0	—	neg.	A.M. A thick moist air.
10	SW	29.90	61	strong	pos.	—	A.M. Near five o'clock P.M. there was

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				
							a fine shower of rain, by which the rod first became electrified negatively, and afterwards positively, with sparks. This charge lasted five hours.
July 11	NW	29.60	61	strong	—	neg.	Half after ten o'clock A.M. large dark clouds frequently passed over the rod, by which it became electrified negatively. When they passed somewhat nearer, and dismissed some large drops of rain, the rod then became highly charged, and the bell rang briskly, and continued its ringing near half an hour. The pith balls were very much agitated, sometimes with a pendulous motion, then started closer or wider asunder several times. But the most extraordinary phænomenon of to-day is, that the rod continued ten hours electrified negatively, without change of kind: during that time the pith balls never closed.
12	N	29.70	63	strong	—	neg.	The operations in the rod to-day and yesterday, when compared, appear to have but little resemblance. This day the electricity has changed four times, and the bell rang briskly for a minute or two between each change of electricity.
13	W	29.62	64	strong	pos.	—	Two o'clock P.M. the wind S. A sudden shower of rain fell, by which the rod became electrified positively, but soon changed to negative. The bell now rang weakly one minute, and then stopped while the electricity changed to positive, which soon changed again to negative. The electricity and the weather have been much varied to-day. At New-Mills, in Scotland, some lightning and thunder.
14	NW	29.57	57	strong	—	neg.	The weather this day has been rather more varied than yesterday, with a continued succession of showers and sunshine: and the atmospheric electricity has been considerably more intense, and constant. There have been five gradual changes of electricity; between four of those changes, the bell rang briskly. The pith balls also exhibited some of their curious agitations; one jerk in

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				
							particular was so very quick and powerful, that the balls were near striking each other. In a few minutes after this, I heard distant thunder. At Burwash, in Sussex, this day a heavy tempest of rain, lightning, and thunder.
July 15	NW	29.78	60	o	pos.	—	The weather these 19 days has been generally moderate; and the electricity of the atmosphere has been only what is common for serene weather, at no time sufficiently strong to afford visible sparks.
16	NW	29.95	63	o	pos.	—	
17	SW	30.13	69	o	pos.	—	
18	W	30.10	66	o	pos.	—	
19	SW	30. 5	65	o	pos.	—	
20	SW	29.75	66	o	pos.	—	
21	W	29.78	63	o	pos.	—	
22	NW	30. 5	61	o	pos.	—	
23	SW	30.	64	o	pos.	—	
24	W	30.	66	o	pos.	—	
25	SW	30.12	69	o	pos.	—	
26	SW	30.15	66	o	pos.	—	
27	W	30.11	67	o	pos.	—	
28	S	30.10	63	o	pos.	—	
29	N	29.65	57	o	pos.	—	
30	NW	29.65	60	o	pos.	—	
31	SW	29.55	63	o	pos.	—	
Aug. 1	NW	29.92	59	o	pos.	—	
2	SW	29.95	61	o	pos.	—	
3	SW	29.77	63	strong	pos.	—	A heavy cloudy morning, but fair. Half after one o'clock P.M. a smart shower of rain fell, by which the rod became highly charged with negative electricity; when the cloud was fully over the rod, the electricity changed to positive; and when it had passed the zenith, the electricity changed again to negative.
4	NW	29.83	62	o	pos.	—	Serene weather, and a weak electricity.
5	SW	30.	67	o	pos.	—	
6	SW	29.93	67	o	pos.	—	
7	SW	30.	72	o	pos.	—	
8	W	29.92	67	o	pos.	—	A heavy cloudy morning, but fair. The electricity of the atmosphere has been so weak to-day, that none could be obtained till after sunset.
9	W	30.	63	o	pos.	—	A.M.
10	SW	30. 5	63	o	pos.	—	Serene weather: the pith balls varying between nothing and three-quarters of an inch open.
11	W	30.10	66	o	pos.	—	
12	SW	30.10	72	o	pos.	—	
13	SW	30. 5	69	o	pos.	—	
14	N	30.10	64	o	pos.	—	
15	S	30. 3	72	o	pos.	—	
16	SW	29.90	73	o	pos.	—	There have been several small showers of

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				rain this day, some of them not sensibly electrified, others were weakly positive, but which I could not distinguish from the common electricity of serene weather, which has prevailed a long time.
Aug. 17	W	30.	66	o	pos.	—	Serene weather; divergency of the pith balls from one to seven-tenths of an inch.
18	N	30.10	66	o	pos.	—	
19	SW	30. 1	66	o	pos.	—	
20	NW	30. 4	76	o	pos.	—	
21	S	29.75	77	strong	pos.	—	A.M. Five o'clock P.M. fell some large drops of rain, by which the rod was highly charged positively; the bell rang briskly about five minutes, then stopped, and the electricity of the rod became negative. The rain now ceased, the sky cleared up, and the electric charge in the rod decreased considerably, and ended as it begun, positive.
22	W	30. 4	62	o	pos.	—	Dark cloudy weather, and a very moist air, attended with a weak electricity.
23	SW	29.95	67	o	—	neg.	
24	W	30.	63	o	pos.	—	
25	SW	29.96	62	o	pos.	—	
26	N	29.70	64	o	pos.	—	
27	N	29.83	62	strong	pos.	—	This day has been very showery, yet only one shower was strongly electrified.
28	W	29.95	59	o	pos.	—	Serene weather, and a constant electricity.
29	NW	30.	60	o	pos.	—	
30	W	30.12	62	o	pos.	—	
31	SW	30.10	66	o	pos.	—	
Sept. 1	W	29.95	67	o	pos.	—	
2	SW	29.80	62	small	pos.	—	A.M. In the afternoon, there was a thick dark sky, with small rain, and a strong wind. On an increase of rain the electricity became negative, with small sparks. When the rain abated, the electricity changed to positive, and so it ended.
3	W	29.50	58	strong	pos.	—	A.M. Near one o'clock P.M. the electric charge in the rod was negative. I then saw thunder-clouds forming in the south, and in the north. Some large drops of rain now fell, by which the charge in the rod became very strong; the bell now rings faintly, and the pith balls exhibit their waving motion, and sometimes sudden jerks. The distinct thunder-cloud, which was in the north, is now gone by the west into the south, during which the charge in the rod be-

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				came weaker and weaker, till it ended as it began, negative. This electric charge of the rod continued near one hour and a half.
Sept. 4	W	29.65	58	strong	pos.	—	A.M. One o'clock P.M. wind NW. Thin white clouds have been passing over the rod; but a distinct black cloud is now approaching, and some rain falls, mixed with hail; the rod is now highly charged with positive electricity; the bell rang loud, and a long time; and as the shower abated, so did the strength of the electric charge abate in the rod. This charge lasted near three hours, without change of kind.
5	NW	29.70	55	small	pos.	—	A quarter before three o'clock P.M. some rain fell, by which the rod became moderately charged positively, but soon changed to a weaker negative. There were two more gradual changes of electricity within the space of one hour.
6	N	30.	58	strong	—	neg.	Three o'clock P.M. a large dark cloud passed over the rod with drizzling rain; its electricity was negative; the bell rang weakly. The electric charge from this cloud began, continued, and ended, negative.
7	N	30.10	51	small	pos.	—	A.M. by the night bottle. Two o'clock P.M. a dark cloud, with drizzling rain, passed over the rod, by which it was electrified positively, with sparks; and ended negative.
8	N	30. 5	57	0	pos.	—	Moderate fine weather, and a weak electricity.
9	SW	30. 8	62	0	pos.	—	
10	NW	30.	58	0	pos.	—	
11	SW	30. 1	51	0	pos.	—	
12	SW	30.10	62	0	pos.	—	
13	W	30.	61	0	pos.	—	
14	S	29.84	62	0	pos.	—	
15	SW	29.80	66	small	pos.	—	The rod has been electrified positively nearly all this day. At five o'clock P.M. an awful darkness covered the heavens, until near six o'clock, succeeded by a drizzling rain, from which I could not obtain (in my common way) any electric fluid: but when the dark cloud became broken, and no rain fell, I then obtained visible sparks at the ball N, which continued until I went to bed;

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				
							and seemed to indicate that it would remain so all night, as the night is perfectly serene, and clear moonlight.
Sept. 16	W	30.	55	small	pos.	—	Five o'clock this morning, I found the rod charged positive, but not quite so strong as when I left it last night; and it gradually went off by nine o'clock, and returned again into the rod at near four o'clock P.M. from which time the fluid gradually increased with the cool of the evening, until visible sparks appeared at the brass ball N. In this state of intensity I left it at near eleven o'clock.
17	W	30.28	50	small	pos.	—	This morning, a little after five o'clock, I found the rod rather stronger charged with the same electricity than when I left it last night; and I have no doubt but it has been electrified all night. The pith balls have never closed this day: their divergency has been between half an inch and two inches. Weather serene.
18	SW	30.10	54	small	pos.	—	Five o'clock A.M. I found the rod electrified positively, and nearly of the same intensity it was in when I left it last night; that is to say, capable of emitting small sparks. The atmosphere this morning is perfectly serene. There is a dew, or low fog, about five or six feet high, spread over the ground in Hyde-Park, very beautiful to look upon from an upper window. For the space of two hours, this vapour gradually increased, and ascended higher and higher, until it quite intercepted the trees from my sight, and became a pale wet fog; which very much injured the insulation of the rod: for during its increase in height, the electricity of the rod decreased, insomuch, that I had almost concluded that the pith balls touched each other. But during this short suspense the sun rose higher, and diminished the fog very fast; and the electricity in the rod soon increased to its former strength; and so continued all day.
19	S	29.85	63	small	pos.	—	Five o'clock A.M. I found the rod electrified positively, but rather weaker than

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	o				
							when I left it last night. The weather this day has been nearly the same with that of the preceding one; only the fog began later, and continued longer. The rod has been electrified all this day.
Sept. 20	W	29.70	57	strong	pos.	—	Near six o'clock A.M. I found the rod electrified, but very weak, barely sufficient to indicate the kind, and it remained so till near three o'clock P.M. The pith balls now closed (which I suppose has not happened before during the space of 92 hours) and opened negative. A heavy cloud from the NNW now approaches the rod, and a little rain falls, by which the rod is highly electrified negatively, and the bell rings briskly; the pith balls diverge to their utmost limit, and exhibit their waving pendulous motion. These operations took up about one hour; and the rod continued electrified negatively the rest of the day.
21	NW	29.90	58	o	—	neg.	By the night bottle. The electricity in the rod barely sensible.
22	W	30.20	52	small	pos.	—	The pith balls have not closed this day. The weather serene.
23	S	29.95	52	small	pos.	—	This morning a quarter before six o'clock, I found the electric charge in the rod much weaker than when I left it last night; which I afterwards found had been occasioned by a spider fixing its web from the rod to the wall of the house; for when the web was removed, the intensity of the electrical charge was soon increased.
24	N	30.30	58	o	pos.	—	Dark cloudy weather, and a strong wind.
25	NW	30.30	58	o	pos.	—	
26	NW	30.38	55	small	pos.	—	Between six and seven o'clock this morning, I found the pith balls quite closed; nor would they even stick to my finger when I touched them. At this instant there was a visible dew spread over the ground in Hyde-Park: I examined it with my hand exploring-rod, and quickly charged an electrometer sufficient to ascertain the kind, which was positive. I now suspected my daily enemies the spiders; I therefore went upon the roof of the house with my

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				
							long feather broom, and gently swept the rod down; on examining the broom, I found a spider's web entangled in the feathers. I then went down to the apparatus, and found the pith balls a quarter of an inch open positive, which gradually increased, even to visible sparks.
Sept. 27	SW	30.35	57	small	pos.	—	Six o'clock A.M. I found the electric charge in the rod much weaker than when I left it last night. Nevertheless, when the sun got up a little above the horizon, the rod became stronger electrified. The pith balls never closed this day.
28	SE	30.12	48	small	pos.	—	The operations of the rod to-day have been nearly the same with the preceding one.
29	N	30.12	55	small	pos.	—	This morning a little before six o'clock, I found the pith balls closed. Atmospheric electricity has been more variable this day than for many days past; sometimes quite null, then weak, or strong by turns, until three o'clock P.M. when it again became constant during the remainder of the day.
30	NE	30.25	58	small	pos.	—	Six o'clock A.M. I found the pith balls open full three quarters of an inch positive, and they continued open all day. Their least divergency (as usual) about midday. The weather still thick and dark in the forenoon; and clear serene weather in the afternoon.
Oct. 1	E	30. 5	52	small	pos.	—	Six o'clock A.M. I found the pith balls opened near one inch and a quarter; and they never closed this day. Their least divergency, as usual, was about midday, and their greatest was near midnight. Weather serene.
2	E	29.84	56	small	pos.	—	Six o'clock A.M. I found the pith balls nearly closed, and the state of the air moist and foggy. Half after seven o'clock, the balls opened a little wider, and continued to increase in strength, so that between eleven and twelve o'clock their divergency was full three inches. I saw nothing in particular to occasion it; and I thought it was above the intensity of common serene weather. I saw indeed a dark cloud, but it was

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				at a very great height. However, this electric charge continued in the rod until seven o'clock P.M. when a shower of rain fell, and the pith balls now closed, and opened negative: a fact which has not happened for eight days. After the rain had ceased, the electricity became again positive.
Oct. 3	SE	29.80	63	0	pos.	—	A wet morning, the air moist, and a weak electricity.
4	NE	29.90	56	small	pos.	—	A very moist air. Half after ten o'clock, fell a heavy rain, which continued three hours. Its electricity was positive, but not strong.
5	SW	30. 2	58	small	pos.	—	A foggy damp morning. The rod has been electrified very near all this day.
6	S	29.85	60	strong	—	neg.	A heavy cloudy morning. Two o'clock P.M. the clouds float in the air lower than in the morning; and the rod is electrified negatively. Four o'clock, a heavy black cloud passed the rod, by which it was strongly electrified, but still negative, and continued so a full hour. During this time there were several sudden jerks exhibited by the pith balls. Near sunset the balls closed, and opened positive, and remained so. In the east part of Kent, some lightning and thunder this day.
7	S	29.85	61	0	pos.	—	A.M. the weather mild and fair.
8	N	29.85	54	small	—	neg.	By the night bottle: by the rod positive.
9	N	29.90	52	0	pos.	—	A.M. Perfectly fine weather.
10	W	30. 4	42	0	pos.	—	The electricity constant this day.
11	SW	30. 8	39	small	pos.	—	The rod electrified all this day.
12	SW	29.86	56	small	pos.	—	A little after six o'clock A.M. I found the rod electrified, but rather weaker than when I left it last night. The weather cloudy, with a thick moist air. One o'clock P.M. fell a sudden shower of rain; the electricity now changed to negative, with small sparks.
13	W	29.75	50	small	pos.	—	A little after six o'clock A.M. I found the rod electrified positively. A warm mizzling rain and moist air. Ten o'clock P.M. balls open two inches.
14	W	29.80	61	0	pos.	—	Six o'clock A.M. balls open half an inch positive.
15	W	30.	59	small	pos.	—	A.M. Fair weather, and strong gales of wind.

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				
Oct. 16	W	30.34	43	small	pos.	—	Six o'clock A.M. balls three-tenths of an inch open. The pith balls have not been unelectrified this day.
17	S	30.15	55	small	pos.	—	During a continuance of moderate weather, I have frequently observed that the atmospherical electricity has periods of increase and decrease of quantity, which are tolerably regular; I shall therefore be accurate in my account of this day's variation, as it may serve for an example of reference to many other days when similar appearances took place. When I left the rod last night, the pith balls were one inch open, positive. Six o'clock this morning, I found them three-tenths of an inch open, positive; and soon after this I perceived them to open wider and wider, so that a little before nine o'clock, the pith balls were stationary at near two inches open. But as the sun advanced towards the meridian, the electric charge in the rod decreased; and it was only possessed of various degrees of a weak intensity until four o'clock P.M.: the pith balls were then only two-tenths of an inch open. Half an hour after four o'clock, they began to increase their divergency, which gradually held on till a little after seven o'clock, when they again became stationary at full two inches open; a little after eight o'clock, they began to decrease; nine o'clock, they were one inch and a quarter open; near eleven o'clock, I left them at a bare inch open.
18	SW	29.90	62	small	pos.	—	Six o'clock, pith balls two-tenths of an inch open, positive. A dark cloudy morning. One o'clock P.M. some rain fell, yet the electric charge in the rod was positive. Near three o'clock, the fall of rain suddenly increased, so did the electric charge in the rod; and at the same time changed to negative, with sparks; and after this ended positive.
19	W	30.15	39	small	pos.	—	The rod has not been unelectrified this day: and the facts and appearances have been nearly like those of the 17th instant.

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				
Oct. 20	E	30. 5	46	small	pos.	—	The operations of the electricity in the rod to-day have been nearly the same with those of the 17th instant.
21	SE	29.91	62	small	pos.	—	The electric charge in the rod has been constant this day; and for particulars may justly be referred to the 17th instant.
22	SE	29.78	59	small	pos.	—	A.M. The pith balls closed and opened, without change of kind, several times this day: and they have been very irregular when compared with many of the preceding. A thin wet fog has prevailed all this day.
23	SE	29.75	53	small	pos.	—	A.M. Four o'clock, P.M. fell a mizzling warm rain, by which the rod became electrified negatively, and emitted small sparks. When the rain was over the rod was electrified positively, and it continued so.
24	NE	29.76	54	small	pos.	—	The rod has been constantly electrified this day, and has been strictly periodical. See 17th instant.
25	N	29.75	54	small	pos.	—	The same as the preceding day.
26	N	29.89	50	small	pos.	—	The operations of the electric fluid in the rod this day have been nearly similar to those of the 17th instant.
27	E	29.90	49	o	pos.	—	Dark moist air, but fair, and some brisk gales of wind.
28	NE	29.73	49	o	pos.	—	
29	NE	29.80	47	o	pos.	—	
30	NE	29.85	44	o	pos.	—	
31	N	29.88	42	small	pos.	—	The thick easterly wind is now over-ruled by a clear northerly one: and the electrification of the rod this day has been of the periodical kind. The pith balls never closed.
Nov. 1	S	29.80	46	strong	pos.	—	Near six o'clock A.M. I found the pith balls open one inch and a quarter, positive; which is rather less than when I left them last night. Half an hour after ten o'clock, fell a heavy shower of rain: the pith balls now closed, and soon opened negative, which, with the rain, continued for the rest of the day.
2	SW	29.45	49	o	pos.	—	P.M. fair weather.
3	SW	29.80	44	strong	pos.	—	From a continued rain of four hours.
4	W	29.74	49	small	pos.	—	Nearly all day, but strongest after sunset.
5	W	29.95	47	o	pos.	—	A.M. and P.M. A cloudy atmosphere.
6	S	29.75	53	o	pos.	—	The electricity of the atmosphere this

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				
							day has been so weak, that I have been obliged to content myself with obtaining the kind by a pair of flaxen threads, without balls to them. The weather warm and showery.
Nov. 7	S	29.68	57	small	pos.	—	The operations in the rod, these two days, have been of the periodical kind. See 17th of October.
8	NE	29.90	42	small	pos.	—	
9	NE	30. 3	44	0	pos.	—	Pith balls not open this day. The kind was obtained by flaxen threads without balls to them. A moist air.
10	NE	30.15	46	0	pos.	—	Sharp dark weather, with a dry easterly wind; and a weak electricity.
11	E	30.	48	0	pos.	—	
12	NE	30.10	47	0	pos.	—	
13	NE	30.15	44	0	pos.	—	
14	NE	30.25	45	0	pos.	—	
15	NE	30.29	42	0	pos.	—	
16	NE	30.16	38	0	pos.	—	
17	E	30.	41	0	pos.	—	
18	N	29.77	33	0	pos.	—	
19	S	29.24	47	strong	—	neg.	The severe easterly wind is now gone, and a more intense atmospheric electricity is returned. Nine o'clock A.M. a large black cloud passed over the rod, and a moderate shower of rain fell, by which the rod was rather strongly electrified negatively. This shower lasted one hour; and near the middle of it, the electricity changed to a strong positive; after this, the electricity ended as it had begun, negative.
20	S	29.25	42	small	—	neg.	By the night bottle. Half an hour after two o'clock P.M. fell some rain, and the rod became electrified negatively. At Calstock, in Cornwall, in the evening of this day, much lightning and thunder.
21	W	29.16	47	strong	—	neg.	Half an hour after midday, fell a moderate shower of rain, attended with a great darkness. The rod now became rather strongly electrified negatively; and the bell rang weakly. There were after this several showers, which only gave a weak positive electricity; except one, which fell at ten o'clock at night; this indeed was strongly positive. At eleven o'clock this night, both at London and Portsmouth, there was much lightning and thunder. And also, at the

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				same time, at Salisbury, where it appeared to roll along the ground like a body of liquid fire.
Nov. 22	SW	29.40	49	0	—	neg.	A.M. The weather showery.
23	NW	29.68	49	0	pos.	—	P.M. A foggy moist air.
24	SW	29.84	37	0	pos.	—	A.M. and P.M.
25	S	29.78	50	0	—	neg.	P.M. Weather showery.
26	NW	29.58	51	0	pos.	—	Atmospheric electricity, these four days, has been governed by its periodical laws. The weather very mild.
27	N	30.20	36	0	pos.	—	
28	N	30.35	39	small	pos.	—	
29	SE	30.12	35	small	pos.	—	
30	N	29.74	32	small	pos.	—	From a shower of snow.
Dec. 1	SE	29.62	32	0	pos.	—	There has been very moderate weather these 12 days. The pith balls have been variable, between null, and one inch open.
2	W	29.42	44	0	pos.	—	
3	S	29.83	45	0	pos.	—	
4	W	29.95	42	0	pos.	—	
5	NW	29.95	32	0	pos.	—	
6	N	30.30	38	0	pos.	—	
7	W	30.30	38	0	pos.	—	
8	N	30.15	49	0	pos.	—	
9	N	30.15	48	0	pos.	—	
10	W	30.18	48	0	pos.	—	
11	SW	29.95	52	0	pos.	—	
12	SW	30. 7	37	0	pos.	—	
13	SW	29.88	48	strong	—	neg.	There has been a constant drizzling rain, with a strong gale of wind, from early in the morning to seven o'clock P.M.; the wind then became a storm. The rod had been null to this time, but now gave signs of a negative charge, and continued increasing its charge until near nine o'clock; at this time the storm seemed to be at its height. The wind now suddenly shifted from SW to N: every appearance is now awful; the pith balls diverge to their utmost limits, and are very much agitated; and the bell rang for half a minute. Half an hour after nine o'clock, the storm ceased; and the charge in the rod became very weak. During the space of two hours the electricity changed six times.
14	W	29.91	39	0	pos.	—	A.M.
15	W	29.40	48	small	—	neg.	From five to eight o'clock A.M. there was a storm of wind and rain. The electricity changed once only.
16	S	29.71	39	0	—	neg.	P.M. A showery day.
17	SW	29.41	41	0	pos.	—	A.M.

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				
Dec. 18	N	28.98	37	small	—	neg.	A.M. and P.M. Strong gales of wind, with rain.
19	S	29.54	33	strong	pos.	—	Between eleven and twelve o'clock A.M. fell a shower of rain, mixed with snow, by which the rod became rather strongly electrified, positively; which continued, with some variation of intensity (without change of kind) near three hours. The greatest divergency of the pith balls was four inches, when fine sparks were received from the brass ball N.
20	N	29.93	32	0	pos.	—	Nearly all day. Divergency three-quarters of an inch. Weather serene.
21	SW	29.90	46	0	pos.	—	The air is extremely moist; and some small rain fell several times this day, which was almost void of the electric fluid; for I could not obtain it in quantity sufficient to ascertain the kind, until I added a *lighted pitch torch* to my hand exploring-rod.
22	W	30. 5	37	0	pos.	—	The pith balls half an inch open nearly all day.
23	W	29.80	44	0	pos.	—	Five o'clock this morning, a most tremendous storm of wind, rain, lightning, and thunder, visited London and other parts. I, being not well, and in bed, did not get up to make observations. I thought I heard the bell ring: however, the rod was but weakly electrified the rest of the day. The high wind continued, without rain, all day.
24	SW	30.20	39	0	pos.	—	Moderate weather, and nearly a constant charge of electricity in the rod.
25	N	30.	45	0	pos.	—	
26	SW	30.22	29	small	pos.	—	
27	S	30. 3	31	0	pos.	—	
28	N	30.	32	0	pos.	—	
29	S	30.20	25	0	pos.	—	
30	S	29.96	37	small	—	neg.	P.M. A fair morning, and a wet evening.
31	N	30. 5	38	0	pos.	—	The rod has been electrified nearly all day, by a fog.
Jan. 1 1791.	S	30. 2	40	0	pos.	—	A.M. In the evening negative, from a fall of rain. This evening much lightning and thunder at Canterbury.
2	SW	29.72	31	small	pos.	—	A.M. A foggy morning. Half an hour after eight o'clock P.M. happened a sudden gale of wind and rain, by which the rod became rather strongly electrified, negatively.

Days.		Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
			Inches.	°				
Jan.	3	SW	29.40	29	small	pos.	—	The morning of this day was frosty, and the evening wet. The electricity of the former was positive, and the latter negative.
	4	SW	29.40	38	0	—	neg.	P.M. Balls three-quarters of an inch open. Strong gales of wind, with rain. Nine o'clock P.M. severe lightning and thunder at Falmouth.
	5	SW	29.29	35	0	pos.	—	Very fine weather. Divergency of the pith balls has been between null and near one inch open.
	6	SW	29.46	37	0	pos.	—	
	7	W	29.18	51	0	pos.	—	
	8	W	29.26	43	0	pos.	—	
	9	W	28.96	42	small	—	neg.	Eight o'clock A M. I saw a large black cloud in the N and NW: soon after, a strong wind suddenly sprang up, and a little rain fell; during which, the rod was rather strongly electrified negatively, with small sparks. This charge of the rod only lasted eighteen minutes.
	10	S	29.75	42	0	pos.	—	Fine weather, and almost a constant electricity.
	11	W	29.75	48	0	pos.	—	
	12	S	29.95	42	0	pos.	—	P.M. A very high wind, and a weak electricity. In the evening of this day, much lightning and thunder at Plymouth.
	13	W	29.48	43	0	—	neg.	P.M. from a shower of rain.
	14	W	29.80	42	0	pos.	—	Very moderate weather, and a weak electricity.
	15	SW	30.	32	0	pos.	—	
	16	SW	29.82	50	0	pos.	—	
	17	SW	29.50	49	0	pos.	—	
	18	S	29.50	42	0	—	neg.	From rain, with a strong wind.
	19	S	28.87	43	0	—	neg.	
	20	SW	28.48	43	0	pos.	—	The weather for these twelve days having been moderate, the electricity of the atmosphere has only been of such intensity as is common to serene weather.
	21	W	29.22	37	0	pos.	—	
	22	S	29.85	35	0	pos.	—	
	23	W	29.76	48	0	pos.	—	
	24	S	30.50	32	0	pos.	—	
	25	SW	30.27	45	0	pos.	—	
	26	SW	30.15	45	0	pos.	—	
	27	SW	29.84	45	0	pos.	—	
	28	N	30.	33	0	pos.	—	
	29	SW	30.	38	0	pos.	—	
	30	NW	29.85	44	0	pos.	—	
	31	NW	29.85	51	0	pos.	—	
Feb.	1	W	29.85	47	small	pos.	—	P.M. from a shower of rain, of large drops; the rod became electrified rather strongly positive, and emitted fine bright sparks at the brass ball N.

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				
Feb. 2	N	30.	38	o	pos.	—	The weather these nine days has been foggy and clear by turns; and the electrification of the atmosphere constant.
3	N	30.20	32	o	pos.	—	
4	N	30.40	32	o	pos.	—	
5	N	30.45	36	o	pos.	—	
6	SW	30.40	36	o	pos.	—	
7	NW	30.32	42	o	pos.	—	
8	W	30.36	43	o	pos.	—	
9	SW	30.32	43	o	pos.	—	
10	SW	30. 8	46	o	pos.	—	
11	SW	29.80	49	o	pos.	—	There has been, these three days, a strong wind, with mizzling rain; and the electricity so weak, that I was obliged to connect, with the hand-rod, *a lighted torch*, to ascertain the kind.
12	W	30.11	36	o	pos.	—	
13	SW	29.95	46	o	pos.	—	
14	SW	29.88	53	o	pos.	—	Fine mild weather, with a constant electricity.
15	SW	29.80	47	o	pos.	—	
16	SW	29.85	43	o	pos.	—	
17	SW	30.10	40	o	pos.	—	
18	SW	29.35	30	o	pos.	—	
19	NE	29.40	37	strong	pos.	—	Five o'clock, a dark cloudy morning, with some small rain; by which the rod became rather strongly electrified, positively. During the passage of one low cloud, the electric charge in the rod changed five times; all of which changes, I have reason to think, were only influentially effected.
20	NE	29.80	40	small	pos.	—	A.M. Rainy dark weather. In the afternoon the charge in the rod became negative, and remained so full three hours, without change of kind.
21	S	29.56	42	o	pos.	—	Dark rainy weather, and a very weak electricity.
22	SW	29.73	32	o	pos.	—	
23	SW	29.55	47	o	pos.	—	
24	W	29.90	33	o	pos.	—	
25	E	30.	37	strong	pos.	—	There has happened this day a continued rain, mixed with snow. The rod has been several times strongly charged; and the electricity gradually changed four times.
26	N	29.72	33	strong	pos.	—	The weather and the electrification of the rod have been nearly the same as in the preceding day.
27	N	30.	38	strong	pos.	—	The weather and the operations of the rod have been nearly the same as in the two preceding days.
28	N	30. 8	36	o	pos.	—	The weather has been foggy these three days.
March 1	N	30.12	35	o	pos.	—	
2	S	30.25	32	o	pos.	—	

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				
March 3	SW	30.50	32	o	pos.	—	Serene fair weather these seventeen days; and the nights were generally frosty. The electricity of the atmosphere has been constant; but too weak to afford visible sparks.
4	SW	30.40	45	o	pos.	—	
5	SW	30.44	47	o	pos.	—	
6	SW	30.20	40	o	pos.	—	
7	NW	30.25	41	o	pos.	—	
8	N	30.55	37	o	pos.	—	
9	SW	30.65	34	o	pos.	—	
10	W	30.55	35	o	pos.	—	
11	SW	30.46	39	o	pos.	—	
12	W	30.28	40	o	pos.	—	
13	SW	30.20	44	o	pos.	—	
14	SW	30.18	47	o	pos.	—	
15	SW	30.35	50	o	pos.	—	
16	SW	30.40	51	o	pos.	—	
17	SW	30.40	40	o	pos.	—	
18	SW	30.28	50	o	pos.	—	
19	SW	30.15	39	o	pos.	—	
20	W	29.68	46	o	—	neg.	This morning fell a warm drizzling rain; which seems to have acted like a charm on the late constant electrification of the atmosphere: for I find it so weakly electrified since the rain fell, that I was obliged to connect, with the rod, *a lighted torch*, to ascertain the kind.
21	NW	29.15	44	strong	pos.	—	Four o'clock this morning, a severe storm of wind and rain, driving full against my bed-room window, induced me to rise from my bed to see the state of the rod; which I found highly electrified, positively. This wind did not abate its force till sun-set. Four o'clock P.M. some rain fell, mixed with snow, which lasted near one hour. During this time the rod was most powerfully electrified positively: I counted seventeen spontaneous explosions between the brass ball and bell, notwithstanding the bell was ringing all the time. Half an hour after five o'clock, came a second shower of rain, hail; and snow, which lasted half an hour, attended with uncommon darkness. During about half this time, the electric charge in the rod was as strong as it could possibly be; for the fluid almost streamed between the brass ball and bell: I counted near two hundred very brilliant explosions between the ball and bell. The bell rang all the time. This latter very high

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				charge was negative. There were four changes of electricity, but all gradual.
Mar. 22	N	29.85	44	o	pos.	—	The disposition of the atmosphere has been mild and regular these four days; and the state of atmospheric electricity constant and periodical.
23	W	30. 3	54	o	pos.	—	
24	N	30.50	44	o	pos.	—	
25	S	30.38	43	o	pos.	—	
26	S	29.85	46	small	—	neg.	Half an hour after nine o'clock A.M. fell some rain, by which the rod became electrified negatively. The wind now became N, and attended with showers of rain the rest of the day: all of them moderately electrified, negatively.
27	N	30.	40	o	pos.	—	The weather has been fair during these ten days, and the atmospheric electricity has been constant; yet it has been too weak to afford visible sparks.
28	NW	30.10	45	o	pos.	—	
29	SW	30. 6	50	o	pos.	—	
30	E	30.	44	o	pos.	—	
31	SE	30. 8	44	o	pos.	—	
April 1	SW	30.	50	o	pos.	—	
2	E	29.81	51	o	pos.	—	
3	E	29 78	50	o	pos.	—	
4	NE	29.90	46	o	pos.	—	
5	NE	29.72	46	o	pos	—	
6	N	29.45	43	small	—	neg.	Six o'clock A.M. I found the night bottle charged sufficiently to explode with a visible spark; the kind, negative. During the night, the weather had changed from dry to a mild rain. The rod has been electrified negatively nearly all this day.
7	W	29.78	47	o	pos.	—	Mild serene weather. The divergency of the pith balls has been varying between null and one inch open.
8	NW	29 92	53	o	pos.	—	
9	NW	29.90	51	o	pos.	—	
10	W	30.	50	o	pos.	—	
11	SE	30.	50	small	—	neg.	Weather still mild, and a constant electricity this day.
12	N	29.95	54	o	pos.	—	From a fog.
13	NE	30.	51	o	pos.	—	Mild serene weather these nine days; some of them foggy: but whether clear or foggy, the vapour in the air has been constantly electrified, positively.
14	W	30.	59	o	pos.	—	
15	SW	30. 6	55	o	pos.	—	
16	SE	30.10	55	o	pos.	—	
17	SE	30.	61	o	pos.	—	
18	SW	29 80	57	o	pos.	—	
19	SW	29 75	59	o	pos.	—	
20	S	29.57	59	o	pos.	—	
21	SW	29 48	56	o	pos.	—	
22	SW	29.25	55	small	—	neg.	A wet morning. Twelve o'clock, the rain fell more plentifully, and the rod was rather strongly charged negatively.
23	SW	29.24	54	strong	—	neg.	Half an hour after twelve o'clock P.M. an

Days.	Wind.	Barom.	Ther.	Sparks.	Pos.	Neg.	
		Inches.	°				
							extensive black cloud, without any breaks in it, approached the rod, by which it became strongly electrified, negatively; and in the space of twelve minutes a most awful darkness filled the atmosphere, and some rain fell, mixed with hail. The electric charge in the rod now became positive; and for the space of ten minutes it was as strongly electrified as it possibly could be; for I now counted 172 spontaneous explosions between the brass ball and bell; the bell ringing briskly all the time. These lucid sparks succeeded each other almost too quick to be noted. When the explosive sparks ceased, the electricity of the rod changed, and ended as it had begun, negative. A little after five o'clock fell a heavy shower of rain, and a second strong charge in the rod succeeded, attended with three gradual changes of kind.
April 24	SW	29.31	54	strong	—	neg.	A little after two o'clock P.M. fell a shower of rain, by which the rod became moderately electrified, negatively. The bell rang weakly. This charge in the rod continued four hours, without change of kind.
25	W	29.84	51	strong	pos.	—	A.M. Three o'clock P.M. fell a shower of rain, which was rather strongly electrified, negatively, attended with three changes of kind.
26	E	30.	53	0	pos.	—	Serene foggy weather.
27	NE	29.84	53	0	pos.	—	
28	N	29.81	52	0	pos.	—	
29	N	29.86	51	strong	—	neg.	A.M. from a shower of rain.
30	N	29.90	48	small	—	neg.	During the greatest part of the day. The weather showery.
May 1	N	29.80	48	small	pos.	—	A.M. Afternoon negative. The weather still showery.
2	N	29.85	48	small	—	neg.	A.M. A mild showery morning.
3	N	30.	45	0	pos.	—	A.M. but very weak. Fair weather to-day.
4	N	30.	45	0	pos.	—	The atmospherical electricity has been so very weak this day, that I was obliged to connect, with the hand-rod, a *lighted torch*, to ascertain the kind of electricity.
5	N	29.96	47	0	pos.	—	Fair serene weather these four days; and the atmospheric electricity constant.
6	N	30.	46	0	pos.	—	
7	N	30.28	45	0	pos.	—	
8	N	30.32	48	0	pos.	—	

A monthly account of sparks, and of positive and negative electricity, as indicated by the pith-ball electrometer, connected with the rod; excepting a few times, in very moist weather, in which it was obtained by the hand exploring-rod, with a lighted *torch* to it. See Fig. 2. Tab. IV.

		Times.		Times.		Days.
23 days of May, 1790, and 8 days of May, 1791,	Positive	40	Negative	27	Sparks drawn	13
June	Positive	45	Negative	22	Sparks drawn	5
July	Positive	36	Negative	23	Sparks drawn	8
August	Positive	33	Negative	6	Sparks drawn	3
September	Positive	39	Negative	11	Sparks drawn	19
October	Positive	37	Negative	7	Sparks drawn	22
November	Positive	30	Negative	8	Sparks drawn	11
December	Positive	35	Negative	11	Sparks drawn	6
January	Positive	28	Negative	8	Sparks drawn	3
February	Positive	36	Negative	12	Sparks drawn	6
March	Positive	34	Negative	8	Sparks drawn	2
April	Positive	30	Negative	14	Sparks drawn	8
		423 times		157 times		106 days

It appears, by comparing the monthly account of this year with that of the preceding, that there has been a considerable disproportion in the electrical positive state of the atmosphere; but which, when duly weighed, will not appear so very great as it now does. For when it is considered, that in the preceding year there were 73 days in which weak signs only of the electric fluid were observed; that seven days were destitute of electric signs; and that that kind of weather in which very weak signs of atmospherical electricity could be obtained, is now found, by a more sensible electrometer than was at that time used, to be always positively electrified, it will, I presume, diminish the apparent disproportion. And as for the remaining difference, I also attribute a good deal of it to the accuracy of my present mode of obtaining atmospherical electricity, with a more complete apparatus; by which I have been able to collect the electric fluid, in sufficient quantity to ascertain the kind which predominates in the atmosphere, even in its weakest state. I have, therefore, found it an easy matter to fix the kind of electricity that the aqueous vapours in the air were charged with in each day throughout the year.

From repeated observations, and long experience, I am perfectly satisfied that the aqueous vapours, suspended in the air, are constantly electrified; requiring only the aid of a proper collector, to render the effects of their electricity at all times sensible. And for this reason, there may be justly said to be, an electrical atmosphere within our aerial atmosphere.

During a course of moderate weather, the electricity of the atmosphere is invariably positive; and exhibits a flux and reflux, which generally causes it to increase and decrease twice in every twenty-four hours. The moments of its greatest force are about two or three hours after the rising, and some time before and after the setting, of the sun; those when it is weakest, are from mid-day to about four o'clock. The periodical electricity of the atmosphere seems to be manifestly influenced by *heat* and *cold*. Hence it plainly appears, why we always find warm small rain to be but weakly electrified; when cold rain, which falls in large drops, is the most intensely electrified of any.

The abovementioned eighth day of May, completes my second year's journal; which, agreeable to my promise, I have now the honour of presenting to the Royal Society, with a description of the improved atmospheric apparatus, which has been used for the foregoing observations.

I must now close that regular account which, for the last two years, has daily occupied my attention. Should any very remarkable appearances, however, in future take place, during a long course of frosty weather, which is the only state of the atmosphere that I have not yet experienced,* as my apparatus will continue in order, I shall carefully note them, and present them to the Royal Society: hoping that some more able philosopher may be induced to take his share of labour in this interesting and promising field, in order to investigate, as far as possible, the nature of so active, universal, and, in all probability, important an agent, as the atmospherical electricity seems to be; and to pursue the subject, with that diligence, and unprejudiced accuracy, which has been my principal object in the course of my observations.

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* As I do not remember that there have been twenty-four hours of hard frost, during the two last winters.

Fig. 1.

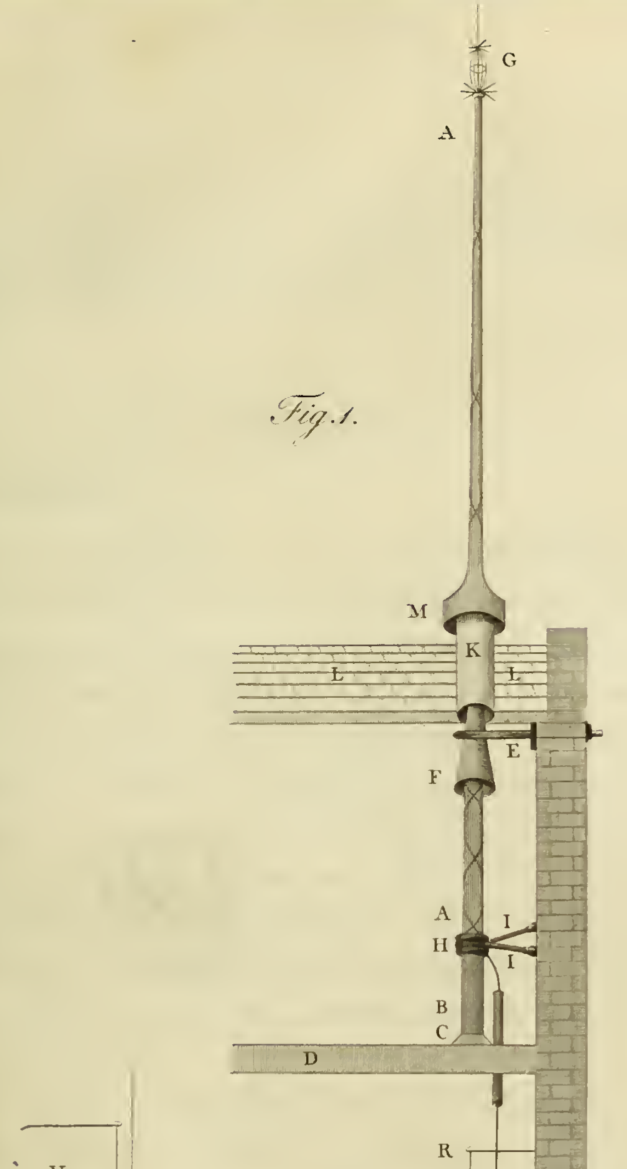
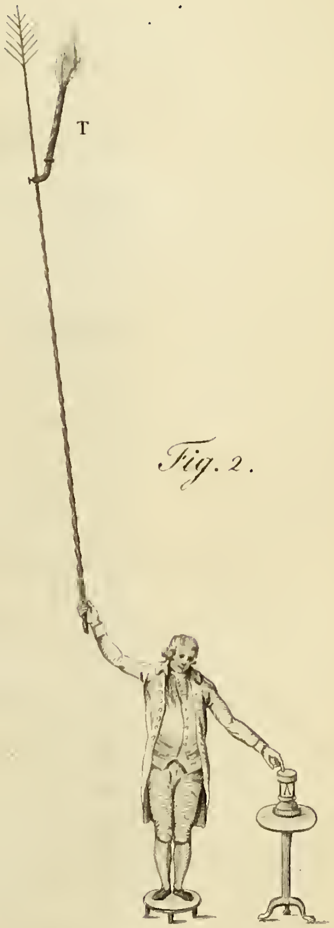


Fig. 2.



Basire Sc.

BRITISH MUSEUM NATURAL HISTORY

XII. *Further Observations on the Process for converting cast into malleable Iron. In a Letter from* Thomas Beddoes, *M. D. to Sir* Joseph Banks, *Bart. P. R. S.*

Read May 3, 1792.

DEAR SIR,

SINCE I described to you the process known among the workmen by the term, *puddling* of iron, I have several times reconsidered the explanation I attempted, in the same letter, of the phænomena it presents. My explanation could not indeed but be in great measure conjectural; and subsequent reflection excited in my mind a very lively wish to ascertain, in a decisive manner, the nature of the process. The following experiments will, I flatter myself, serve to determine the degree of confidence with which the principal points of my theory may be received, though they will not afford a solution of all the questions which my former communication might suggest to an acute philosopher.

They were undertaken in order to ascertain, 1. whether any elastic fluids are really extricated during the conversion of cast into malleable iron; and 2. what is their nature; and 3. whether they vary at different periods of the process, as I concluded from the appearances in the furnace. It seemed of less consequence to ascertain their quantity. I did not, however, neglect this object of inquiry, but you will find that some very curious circumstances prevented me from attaining it.

EXPERIMENT I.

Six pounds of dark grey melting cast iron were put into an earthen retort; a glass tube was luted to the neck, and its extremity was immersed in water. The retort was placed in a wind furnace. Before the retort and its contents could be supposed to be red hot, inflammable air came over. It burned with a deep blue flame, and was in no degree explosive. It rendered lime-water turbid, and was partly absorbed. When the retort had been heated about an hour and half, the air, which was coming over pretty copiously, that is, at the rate of an ounce measure every three minutes, upon an average, suddenly ceased, and the apparatus, on examination, proved to be no longer air-tight. The retort was found to be cracked; and the lumps of iron had none of them been melted, but they had been softened, and conglutinated together.

EXPERIMENT II.

Four ounces Troy of the same iron were put into one of Mr. WEDGWOOD's earthen tubes, glazed and closed at one end. That end of the tube was inclosed in a barrel-shaped crucible, the interstice filled with sand, and the crucible reclined so as to form a very small angle with the horizon: in other respects the apparatus was disposed as before. On the application of heat, air was again extricated, sooner than I should have expected, of the same inexplosive inflammable kind. About one-seventh of that which came over first, and which traversed the water of the receiving vessels, was absorbed by milk of lime. The residue burned slowly, with a flame apparently not so deep as before the carbonic acid was separated.

In this and the former experiment, the elastic fluids were most rapidly extricated on the first impression of a red or white heat. Afterwards they came over much more slowly; during a considerable part of this experiment you might count twelve slowly between every air-bubble.

When the utmost power of the furnace had been exerted for three hours, a phænomenon occurred which produced some surprise in every person present; and there were several who had been abundantly accustomed both to chymical and metallurgical operations. A considerable absorption took place, and for about half an hour, it was necessary to blow air up the glass tube, to prevent the water from rising into contact with the iron. It afterwards appeared that the lead of the glazing was revived, which sufficiently explains the absorption.

586 grains only of the iron had been completely fused. The surface of two of the unmelted lumps was curiously covered with numerous small blisters of metallic lead.

About seven hours after the fire was first kindled, it was discovered that the apparatus had failed. I had examined the air that came over immediately before this accident, both by means of lime-water, and milk of lime, without discovering any vestige of carbonic air.

The iron weighed altogether three grains more than at first. But the adhering lead, and a quantity of lead also which was incorporated with the iron, concealed a real, and probably a considerable, loss of weight. The phænomena it exhibited, when put into weak vitriolic acid, and the vitriolated lead which was formed, indicated the presence of this metal in all the superficial parts of the mass. When it had been kept some time in vinegar, it dissolved readily enough in vitriolic acid at first, but the solution soon ceased, or became very slow.

EXPERIMENT III.

A coated flint glass retort was employed in this instance. The apparatus resisted a strong heat for two hours; and air, of the heavy inflammable kind, came constantly over.

EXPERIMENT IV.

A coated retort of crown glass, containing six ounces Troy of the same iron, was placed on a crucible nearly full of sand, and disposed as in the former experiments. I now wished to measure the quantity of the air, and I therefore determined to receive it in mercury. It would have been in vain to attempt this in water, on account of the carbonic acid air. About twelve o'clock the retort was judged to be of a dull red heat, and inflammable air came over. The orifice of the transmitting glass tube was now covered to the depth of half an inch with mercury, when the discharge of air instantly ceased: the lute seemed entire. Some of the mercury being removed, so as to leave just enough to cover the mouth of the tube, immediately the air issued again in bubbles, a proof that the apparatus was entire. The mercury was poured into the trough again, and in an instant there was a cessation of air. The mouth of the tube being uncovered, and a lighted paper applied, a blue flame appeared, and continued to burn, so great was the quantity of air discharged. The orifice of the tube was one-tenth of an inch in diameter. We found that this constant flame could be produced at any time during three hours and an half. When water was substituted in place of mercury, air issued slowly, and as if with difficulty, under a pressure of five inches. When only half an inch was left over the mouth of the tube, small bubbles ascended freely. During a considerable time I counted four, slowly, between each of these bubbles. I did not collect above three ounce measures of air, and this contained carbonic acid. It was past four o'clock when the apparatus ceased to be air-tight, and the fire had been kept as strong as possible. The iron was most completely fused. There was a good deal of revived lead within the retort; there were also many globules in the neck. Probably some broken flint glass had been added to the usual materials for crown glass; I cannot otherwise account for the appearance of the lead here. In the last experiment the lead of the flint glass had been revived.

EXPERIMENT V.

Two ounces of the same iron, immediately upon being taken out of a retort, in which they had been kept, at a red heat, for about an hour and a half, and which were therefore as free from water as iron can easily be procured, were put into an earthen tube, unglazed, and closed at one end. This tube was disposed as in experiment II, only the end of the glass tube was immersed in mercury, instead of water. You will not be surprised to hear that air did not now come over so soon as in any former instance. When the fire was raised to its full force, exactly the same amusing variety of appearances took place as in the last experiment. Under the pressure of half an inch of mercury, not a particle of air was discharged; but the moment the pressure was diminished to a small fraction of an inch, the bubbles succeeded each other pretty quickly; and so on repeatedly. Upon lowering the surface of the mercury, and pouring some water upon it, I received more than two ounce measures of air, which, by the test of lime-water, did seem to contain a vestige of carbonic acid, but it was too minute to be appreciated. This experiment with the air was made after a strong white heat had been kept up for three hours. Soon afterwards the bubbles ceased; but we could not then, nor upon examination of the apparatus when cold, discover any failure. The fire was still kept up for three hours. The tube must have been exposed to a strong white heat seven hours in all. The iron had lost eleven grains in weight. Only about one half had been thoroughly fused. The surface of two lumps, that had not been fused, had the close texture, and silvery appearance, of malleable iron. The thin edges yielded to the stroke of the hammer, and a gentleman, perfectly conversant in the nature of iron, agreed with me, that it had all the characters of malleable iron.

EXPERIMENT VI.

Thirty-one grains of artificial plumbago, in shining flakes from the iron works, were exposed in a small retort to a strong heat, for six hours, in the same pneumatic apparatus. It was difficult to separate, even by the help of the magnet, all the intimately mixed particles of iron, and there were also a few particles of coak incorporated with the plumbago. Air, of an explosive inflammable kind, was extricated, and rose freely through five inches of mercury. We had not been sufficiently careful to let the lute fix before we commenced the experiment, and it soon failed. Upon taking off the pressure of the mercury entirely, and repairing the lute as well as we could, we had every reason to believe that the air soon ceased. The air received in the mercury contained one-eighth of carbonic acid. The remainder exploded. The plumbago lost four grains. Mr. Pelletier, if I remember right, found that native plumbago, exposed to a fierce and long continued heat, lost 10 grains in 200. In the present experiment, its appearance was unaltered. Probably the loss was owing to moisture imbibed by the particles of coak, and to a small combustion by the air in the retort.

It will, I think, be admitted, that these experiments abundantly confirm the inferences I had formerly drawn from appearances by their nature less decisive. The real extrication of air, varying in its nature at various periods of the process, seems to be placed beyond doubt. The experiments in glazed and glass vessels, were made with a view to exclude the possibility of the supposition of the air entering through the pores. I think that Dr. Priestley, if he should repeat these experiments, and find that they have been accurately made, will, with his accustomed openness to conviction, abandon an opinion he has for some time entertained, and no longer consider water as essential to the constitution of elastic fluids. Several observations might be made upon this point, and those which I have just noticed above; but they will readily occur to persons conversant in chymistry, and it is not the object of the publications of the Royal Society to teach the elements of science. I shall, therefore, confine myself to the unexpected and anomalous appearances, and then attempt to draw a few useful inferences.

1. I was surprised at the extrication of inflammable air in such low degrees of heat. You have seen that cast iron, highly charged with charcoal (the *phlogisto onustum* of Bergman) yields air at the temperature of melting lead. For undoubtedly the blisters of lead, which lay upon the iron, are to

be considered as air bubbles caught in a solid film of lead. Perhaps white cast iron would not yield air so readily; possibly iron holds its charcoal with more force as it contains less. I intend to make some comparative experiments upon the varieties of cast iron, but so curious an appearance as these blisters, will always be rather the bounty of accident, than the effect of skill or labour. The obvious method to produce them, would be to cover the iron with lead. All the unmelted lumps of iron were porous, and open in their texture.

2. I am at some loss how to explain the occasional discharge and cessation of air, in one experiment in which a crown glass retort was used, and in another with an unglazed earthen tube. There was no flaw in the lute, nor in the vessels, for it was discharged for the space of several hours under a small pressure. Either, then, it was forced through the softened glass in the first, and the dilated pores of the tube in the second case; or it was absorbed by the substance of the vessels; or it was not extricated from the iron. Of these suppositions the third seems to me the most probable. It is not likely that an hole should be made through the melted glass, under the pressure of the half, and closed under that of perhaps the eighth of an inch; or that pores in the tube should open and shut in conformity to such a variation of circumstances: and, with regard to the tube, there can be no question as to absorption. One principal difficulty, as it appears to me, in the manufacture of iron, is to get rid of the charcoal. The oxygène readily enough unites with a small portion; but the attraction of the iron on the one hand, and on the other, the little disposition of the charcoal to put on the elastic form, in comparison with many other less fixed substances, together form a

very considerable obstacle to the change of charcoal into air; and as I have already observed, the iron probably holds the charcoal more strongly as its quantity diminishes. In this state of things a small additional impediment will prevent the heat from throwing the charcoal into the state of air; and some degree of pressure must be adequate to this effect: and why may not this point, from which as you recede on opposite sides, the attraction of the particles of charcoal for one another, or for iron, either shall or shall not be overcome by heat, have been found in these experiments? The next consideration will both illustrate and confirm these ideas.

3. A chymist, whose notions of iron are derived principally from books, and from the phænomena which are presented by processes not having metallurgy for their immediate object, will be apt to consider some things related above as inconsistent: the violence of the heat, for instance, and the smallness of its effects; since even cast iron was not fused in all the experiments. The fact is, when cast iron exposes a large surface, and heat is gradually applied, it proves almost as infusible as malleable iron: indeed, by the gradual action of heat it is converted, superficially at least, into malleable iron, or approximates towards it: and considering only iron and charcoal, I believe, *the fusibility of iron will be directly as the quantity of charcoal it contains.* Now in the experiments I have described, pieces of one, two, and three drachms, and sometimes less, were used, for larger could not be inserted into the neck of the retort. And, in order to avoid this inconvenience in future, I would recommend cylinders to be cast, of a diameter suited to the mouths of the vessels. This infusible coat would be an impediment to the conversion of the parts below, by

pressing upon them; the elastic fluids could not either traverse the solid surface so freely as a liquid, and perhaps, as I am disposed to believe, they could not traverse it at all. The malleable skin seems close in its texture, and the porosity of the rest might arise from the generation of just air enough to produce an internal expansion. In the puddling operation, it is of the most material consequence to keep the mass in constant agitation. Thus the parts are thoroughly blended, the attraction of cohesion is a good deal counteracted, and there can be no pieces hide-bound, if I may so express myself. This last, perhaps, is the greatest advantage derived from the labour of the workman.

4. I was asked by one of the most ingenious and profound philosophers of the present age, why I had neglected the action of the atmospheric air in the theory of the conversion of iron? It is simply because its action upon the metal seems, in practice, pernicious; I consider its presence as an evil, though a necessary one, according to the present modes of working; I was also anxious to try this opinion by the test of experiment, and you see it has been fully confirmed. In the last experiment, part of the iron was completely converted, and in some others it seemed approaching fast towards *nature*, as the manufacturers express it. It is, indeed, very possible to conceive a way in which air might be beneficial; that is, if it could be applied so as to burn the charcoal merely; but at present, for one grain of charcoal which it converts into carbonic acid air, it converts many of iron into finery cinder; and as I have formerly shewn, this is not the way in which iron is actually converted in the reverberatory, and probably not in the finery, furnace.

5. It is impossible to ascertain the principles of any art, without immediately improving the practice, or opening a prospect of future improvement. The preceding observations may serve to direct attempts to render the metallurgy of iron less difficult, laborious, and expensive. For, 1. if a quantity of oxygène, nearly sufficient to burn the charcoal, could be chymically combined with the cast iron, the operation would consume less fuel, and would not require so long a time. It may be worth while to consider if the ores of iron, containing manganese, owe any part of their value to this circumstance. 2. If it could be contrived to apply a sufficient heat to large quantities of iron in close vessels, and at the same time, to agitate them sufficiently, the loss in conversion would not, perhaps, exceed ten in an hundred. 3. The important object of converting British iron into steel, may possibly be attained by following up reflections suggested by the foregoing experiments. When the oxygène has been separated in the form of carbonic acid, there will remain the charcoal and iron, the constituent parts of steel. Perhaps the materials, at a certain period of the process, may be so nearly approaching to steel as to be easily convertible. The mass will contain also a quantity of sulphur, on which perhaps the difficulty of making good steel from our iron depends. But this difficulty, I am persuaded, will not be insuperable.

It may be proper to add, that whenever attention was paid to it, the hepatic smell in the extricated air was perfectly distinguishable.

I hope you will also permit me to add, that whatever information or advantage may be derived from these facts and observations, must be in a great measure ascribed to the liberal curiosity of William Reynolds, whose enterprising spirit and inventive genius have improved our machinery, enlarged our manufactures, and changed the face of a large district in his native county.

I have the honour to be, &c.

Shifnal, Shropshire,
February 14th, 1792.

THOMAS BEDDOES.

P.S. The residuum of 486 grains of cast iron, the same as that used in experiment 1. weighed 48½ grains, after being dissolved in weak vitriolic acid, and heated to a dull red heat; the same quantity of iron, after the experiment, afforded a residuum of 39 grains, and a little more; in the residuum left by equal quantities of iron, before and after the experiment in the unglazed tube, there was a difference of five grains; the solution of the iron that had been submitted to the experiment went on very slowly; and would not have been effected by vitriolic acid in many months. In the latter case, I used some muriatic acid, which quickly dissolved it: in the former, weak aqua regia was used for the solution of a very small part of the whole lump. I suspected lead to have caused the slowness of the solution in the first case, but there can be no such suspicion in the second. The difference between these residuums tends to shew that plumbago was consumed by the heat: but they do not shew the loss accurately; for in the residuum of the iron that had been fused in the first experiment, there was a small quantity of vitriolated lead; and in the other there was, besides the plumbago, a small quantity of that difficultly soluble calx of iron, which the solutions of this metal deposit on long exposure to air. The difference was greater, therefore, than it appeared. On the other hand, the long action of the acids might have consumed some plumbago. There was little or no calx attractable by the magnet in the residuums of the fused iron. From the 48 grains of residuum, I separated more than six grains by the magnet.

XIII. *Continuation of a Paper on the Production of Light and Heat from different Bodies. By Mr.* Thomas Wedgwood; *communicated by Sir* Joseph Banks, *Bart. P.R.S.*

Read May 10, 1792.

EXEPRIMENT I.

In order to discover what effect the *light* of the burning fuel has upon incombustible bodies, I fixed into the end of a tube of earthen ware * two equal cylinders of silver, with polished surfaces, half an inch in length, and a quarter of an inch in diameter (see Tab. V. Fig. 1.); one of the cylinders was painted over, except the end within the tube, with a thin coat of incombustible black colour, to make it *absorb* the incident light; the other, intended to *reflect*, was left with its polished surface. Applying my eye to the opposite extremity of the tube (which it fitted exactly, so that no extraneous light could enter), and directing it towards the two polished ends of the cylinders, I held the tube within a red hot crucible, surrounded by burning coaks, and continually turned it round, that both cylinders might be equally exposed to the light, and heat. The result was, that the end of the blackened cylinder began to shine a considerable time before that of the polished one, and remained constantly somewhat brighter: on removing the tube from the crucible, still looking within it, I was surprised to see the appearance reversed, the polished cylinder continuing to shine for some time after the blackened one had ceased. Cylinders of gold, and of iron, treated in the same manner, gave the same general result; but the differences between the polished and the blackened ones were not so remarkable in these, as in the silver.

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* When *earthen ware* is mentioned in this paper, the cream-coloured or queen's ware is meant.

I repeated this experiment many times, and found, by observations with a stop-watch, that the blackened silver cylinder began to shine, at a medium, in two-thirds of the time which the polished one required; and that, after its removal from the crucible, it continued to shine only two-thirds of the time that the other did. For this latter observation, I was obliged to make a little variation in the apparatus; the tube itself becoming frequently so hot, as to make the cylinders continue longer red than they otherwise would have done: I therefore took them out of the tube, to suspend them by a fine wire, and then heating them in the ignited crucible as equally as possible (for they cannot be made to exhibit to the eye the same precise tinge of redness), I removed them immediately into a dark place.

From this experiment it would seem, that a great part of the *light* emitted by the cylinders was absorbed from the red hot crucible, as the blackened one, which absorbs most rays, not only became first red, but likewise shone brightest. The following experiment, however, affords a different conclusion.

EXPERIMENT II.

An earthen ware pipe, of a zig-zag form (Fig. 2.), was placed in a crucible, which was filled up with sand, the two open ends of the pipe being left uncovered; one of them was of a proper form for receiving the nozzle of a pair of bellows, the other bent into angles of the form of the letter Z: on this last was fastened a globular vessel A, with a lateral bent pipe, to let out air but exclude all external light, and with a neck in which was inserted a circular plate of glass. The crucible, with the sand and the part of the pipe contained in it, was then heated to redness. Having my eye fixed in the neck of the vessel A, and observing it perfectly dark within, I directed an assistant to blow with the bellows. The stream of air, sent through the red hot tube, not being at all luminous, I fixed a small strip of gold into the orifice of the tube at B, which, after two or three blasts, became faintly red; thus proving, that the air, though not luminous, was equal in temperature to what is usually called red heat. I then heated the crucible to a brighter redness: the stream of air, blown through the bright red hot tube, still came out perfectly dark, but the strip of gold, exposed to it, shone both *sooner* and *brighter* than before.

Hence it appears, that the greater brightness of the blackened cylinder, in the first experiment, was owing to its being of a higher temperature; and that it would have been equally bright had it been raised to the same temperature by any other means than the absorption of *light*; the metal being here brought to a faint, and to a bright ignition, without the access of any visible light.

But perhaps another consequence may be fairly drawn from this experiment. As the gold may be made to emit *light* for any length of time, by being supplied with *heat* from the *dark* air of the temperature of red heat, neither the gold nor the air suffering any chemical change whatever, is not the *light emitted* identical with the *heat received?* This identity appears to be confirmed by the following observation: that if the solar rays be made to converge upon one end of a blackened cylinder of metal, the other parts will become red hot, and emit light; or, if the rays be converged upon the tube blackened, and air passed through it, the gold placed in the dark current will yield a constant light.

The simultaneous *absorption* and *emission* of light, in a red hot body, is a subject of very difficult and abstruse investigation, as it involves the nature of the constituent parts of matter, and of their relative actions and arrangements. I shall not attempt to offer any hypothesis for explaining the various phænomena, as I have not been able to form one at all satisfactory to myself; but shall proceed to state a few miscellaneous experiments and observations, which, though apparently unconnected, may yet be of some assistance to the speculation of some abler theorist.

EXPERIMENT III.

A quart of oil was poured into a bright tin vessel, which had a FAHRENHEIT's thermometer fixed in its neck. The mercury standing at 45°, the vessel was plunged into boiling water, and the time which elapsed before the mercury rose to 180° was exactly noted. I then blackened the exterior surface of the tin vessel, and, repeating the experiment, found the thermometer to require exactly the same time as before, to rise to the same degree.

From the foregoing experiment it appears, that *black* matter has no particular attraction to light in a *quiescent* state, that is, when combined, as heat, with other matter.

EXPERIMENT IV.

Three equal cylinders of glazed earthen ware were fixed in the end of a tube (like the two silver ones in Fig. 1.); one of them blackened; another gilt, all but the ends within the tube; and the third with its glassy surface. These, treated in the same manner as the silver cylinders, in the first experiment, all became *red* at the *same time*. Without taking them out of the tube, I removed the whole from the fire, and, still keeping my eye upon their ends, observed them all to disappear together.

To account for the simultaneous ignition of these three cylinders, it must be considered, that earthen ware being a very slow conductor of heat, the surfaces of all of them are, probably, heated to redness some time before any such appearance takes place in the ends within the tube. Now it is not unlikely, that the black matter, the gold, and the glaze, when red hot, may reflect light equally; and, in that case, there should be no perceptible difference in the time of ignition of the ends within the tube, except the little advantage gained by the blackened one before its black coat becomes red hot, which is partly counterbalanced by the powdery matter (of which the coat consists) obstructing the transmission of heat. The surfaces of the silver cylinders (experiment 1.) on the contrary, do not become red any sensible time before the ends within the tube, the metal conducting the heat rapidly through its whole mass.

An earthen ware cylinder, fastened in the end of an earthen ware tube, along with a gold one, and plunged into melted glass, is much longer in heating than the gold one. This is easily explained, upon a well known principle, namely, that in two bodies of unequal temperature, the colder body conducts the heat from the hotter at a rate directly proportionate to their difference of temperature. Now the surface of the earthen ware cylinder, as the heat is conducted very slowly from it by the interior mass, soon becomes very nearly of the temperature of the hot glass contiguous to it. The surface of the gold cylinder, on the contrary, having its heat conducted from it much faster by the interior mass, is of consequence disposed to receive the heat with greater rapidity.

EXPERIMENT V.

Equal pieces of gold, silver, copper, and iron, blackened all over, and suspended by a wire in a red hot crucible, became red in the order in which they are here set down; and when made equally red, and removed into the dark, they disappeared in the same order. When just brought out of the fire, they all looked equally red; but when they had cooled a little, the iron was much the brightest.

An earthen ware cylinder, tried with the metals, disappeared much sooner than any of them, the interior part not communicating its heat quick enough to keep the surface of the temperature of red heat: accordingly, when broken, though the surface gave no light, the mass was luminous internally.

From a parity of reasoning, a gilt earthen ware cylinder, suspended in a red hot crucible along with a gold one, would probably become red on the surface before the gold one.

EXPERIMENT VI.

A tube of unglazed earthen ware, open at top, and having one half of its bottom blackened on the outside, was placed in a red hot crucible, and the eye directed, as before, to the inside: the part which was externally blackened became always red before the other.

The experiment was repeated with a metalline tube; but no difference could here be perceived between the blackened and unblackened half of the bottom. The reason is obvious, from the foregoing observations.

EXPERIMENT VII.

To ascertain whether metals and earthy bodies *begin* to shine at the same temperature, I gilded, in lines running across, a thin piece of earthen ware, of the specific gravity of about 2,000, and luted it to the end of a tube, the gilt side being inwards; then, directing my eye into the tube, I held it within a crucible, which was gradually made red hot; but I could not, after many trials, perceive that either the gold or the earthen ware began to shine first.

As it appears, from this experiment, that gold and earthen ware begin to shine at the same temperature; and as no two bodies can well be more different, in all their sensible properties, may it not be inferred, that almost all bodies begin to shine at the same temperature?

EXPERIMENT VIII.

Observing that colourless transparent glass had a paler hue, when red hot, than most other bodies, I conceived that it might not be luminous at so low a temperature. I therefore took a circular piece of glass, about $\frac{1}{40}$ of an inch thick, and having gilt one side of it, exposed the ungilt side to a stream of air passed through a red hot tube; but did not perceive that the gold shone at all before the glass. This experiment, however, is not decisive; glass being so slow a conductor of heat, that its exterior surface might have been heated some time before the interior, and thus have deceived the eye. I could not meet with any glass sufficiently thin for this purpose, nor think of any other possible mode of trial.

EXPERIMENT IX.

Having often remarked, that the surfaces of red hot metals had an appearance different from what they present by reflected light when cold, I had an idea that this peculiar appearance might be derived from a transmission of the light through the superficial parts of the ignited body. To ascertain whether they acquired any degree of transparency by heat, I fixed a circular plate of fine gold, about $\frac{1}{60}$ of an inch thick, on the end of a tube, which was perfectly closed by it; then, having heated it to redness, and looking down into the tube, I pressed the outer surface of the gold against single grains of gunpowder: the red light of the gold looked whiter on every flash. To be satisfied that no light found admission through the sides of the tube (which were of thick earthen ware), I covered the exterior surface of the gold plate with a thick coat of clay luting, and again making it red hot, fired gunpowder with it as before, but no increase of light was now perceptible from the flash; which proves, that the sides of the tube were impervious to the light. When this gold was cold, I stuck a few grains of gunpowder upon its surface, and looking within the tube, fired them by pressing them against a hot iron, but the light of the explosions was not then sensible.

Plates of silver, and of iron, gave the same results.

EXPERIMENT X.

A lump of the most luminous marble, and an equal lump of the same marble blackened over, were placed together upon a mass of iron heated just under redness: the former gave out much light, the latter none. Upon a second exposure, the lump not blackened gave a faint light; the blackened one, as before, none at all. Then wiping off the black, and placing them together upon the heater, I found the one which had been blackened to emit as little light as the other: thus the phosphorescent property was nearly destroyed, without any visible light leaving the body.

EXPERIMENT XI.

If a piece of glass, or glazed or unglazed earthen ware, with any enamel, painting, gilding, or writing in ink upon it, be made red hot, the coloured parts appear considerably more red than the others, and continue longer visible. Iron wire, within a red hot glass tube, looks much more red than the glass. Black matter, upon a large polished mass of fine gold, did not remain any longer red than the gold.

EXPERIMENT XII.

A bit of iron wire becomes visibly red hot when immersed in melted glass. Air, therefore, is not necessary to the shining of ignited bodies.

EXPERIMENT XIII.

A piece of red hot metal continues to shine for some time after its removal from the fire ; which proves, that constant accessions of light or heat are not necessary to the shining of ignited bodies. If the piece be strongly blown upon, it instantly ceases to shine ; for the cold air, continually applied, unites with the light as fast as it leaves the body, and which otherwise would have passed to the eye.

I shall now close this paper with two or three miscellaneous observations.

Red hot bodies, though ignited by *white* light, give out only the *red* rays. Perhaps the other more *refrangible* rays, from their greater attraction to matter, may be circulating as heat, whilst the red ones, having a less attraction, yield sooner to that force which propels the light of red hot bodies. If the intensity of the incident white light be much increased, so as to raise the body to a white heat, the more refrangible rays then come out with the others, constituting together a white light.

The flash of a grain of gunpowder is a pure *white* light : but if the explosion be made within a thin, unglazed, earthen ware tube, close at both ends, all the light that pervades the sides of the tube is red : the other rays must, therefore, remain united with the matter of the tube, whilst the less attractive red ones are transmitted. Thus also, on looking at the sun through the thin bottom of an earthen ware tea-cup, only the red rays are transmitted, so that the others must be retained by the matter of the cup.

It would perhaps be worth trying, whether a body can be made *red* hot by concentrated rays of other colours.

The light produced from bodies by attrition consists of a double light; that which their powder would give out on the heater under redness; and that which particles in their surfaces give out by being made red hot. The sudden heating of a body to redness, by a single rub or blow, is a remarkable phænomenon, and deserves to be investigated. One effect produced upon a body by attrition, is a compression or condensation of the parts in its surface; and it appears from general observation, that a condensation of the parts occasions a diminution of its capacity for heat. Iron may be made red hot by repeated blows of a hammer; and I have found, that if red hot iron be forcibly struck by a heavy hammer, with a sharp edge to concentrate the action, the part so struck emits a white light for a sensible time, and is probably raised to a white heat: also, that my father's thermometer clay has its capacity for heat diminished one-third, by being burnt to 120° of his scale, and thus reduced to about one-half of its bulk; and as it loses in weight little more than two grains on a pound, the diminution of capacity can only be attributed to its condensation. Many other analagous instances might be adduced if necessary;* but these will, perhaps, be deemed sufficient to render it probable, that the sudden ignition of the particles by attrition proceeds from the compression, and consequent diminution of the capacity for heat.

I am not certain that the increase of brightness in the gold plate, experiment IX. must be attributed to its transparency: it may arise from the gold being suddenly heated to a white heat by the light of the explosion; or the force of the explosion may condense its parts, and diminish its capacity for heat or light. There is, however, a strong analogical argument for the transparency of the gold: every body whatever, when extremely thin, is pervious to light in such quantity as to be perceptible to our eye-sight: thus gold, perhaps the most opake of all bodies, platina excepted, when beaten into leaf gold, is so pervious to the green rays, that, if held close to the eye, all objects are seen through it with considerable distinctness, appearing of a deepish green hue. Now the particles of matter in the gold plate being much separated from one another, if not more regularly arranged, by the heat; and the intensity of the light in the explosion of the grains of gunpowder being so great; it is not improbable that some few rays may be transmitted through the gold.

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* See Dr. Darwin's excellent paper of Frigorific Experiments on the mechanical Expansion of Air, &c. Phil. Trans. Vol. LXXVIII. p. 43.

After some reflection upon the curious result of experiment 1, I am inclined to think, that the blackened cylinder does not begin to shine at so low a temperature as the polished one; and, consequently, that the *commencement* of ignition is not, in all cases, a certain indication of a particular temperature. For, when the two cylinders were removed from the ignited crucible (see Fig. 1.) the blackened one looked of a brighter red than the polished, and yet, in the course of cooling, disappeared in about two-thirds of the time that the polished one continued to shine, without any apparent reason for its cooling at a faster rate. Should it not, therefore, seem that it requires a greater heat to make it shine?

I am well aware, that these appearances may be differently explained; and, to determine this point, I would propose the following experiment. Put larger cylinders into the tube; and, having made them red hot, drop them separately, each at the instant of its disappearing, into cups of weighed water, of the temperature of between 211 and 212° of FAHRENHEIT: then, as any addition of heat will expand the water into steam, the loss of weight of each vessel will give an exact measure of the heat of the cylinders at the time of immersion.

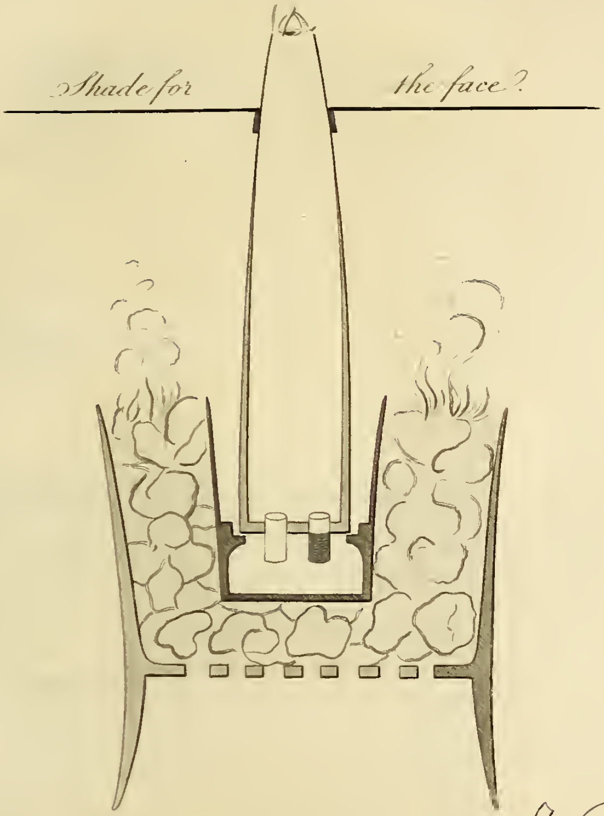

Fig. 1.

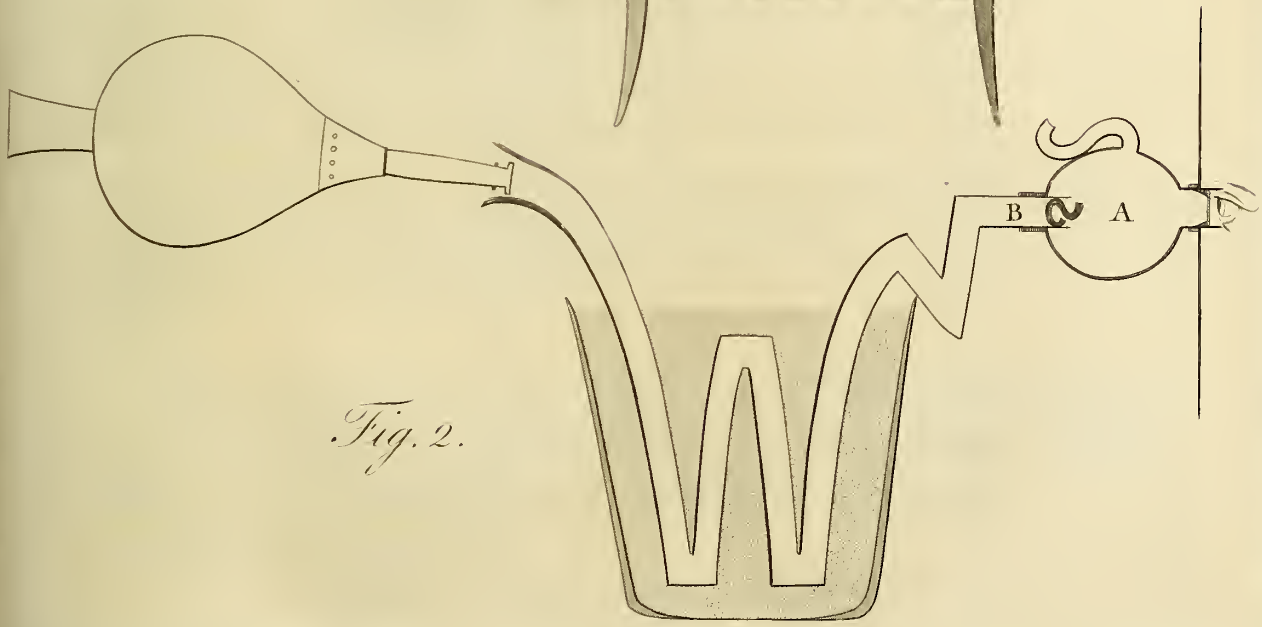

Fig. 2.

BRITISH MUSEUM
NATURAL HISTORY.

XIV. *A Narrative of the Earthquake felt in* Lincolnshire, *and the neighbouring Counties, on the 25th of* February, 1792. *In a Letter from* Edmund Turnor, *Esq. F.R.S. to Sir* Joseph Banks, *Bart. P.R.S.*

Read May 10, 1792.

DEAR SIR,

When I heard of the earthquake of the 25th of February last, which was felt in Lincolnshire, and some of the neighbouring counties, about a quarter before nine in the evening, I immediately wrote to my friend Captain Pennyman, at Little Paunton, near Grantham, desiring him to inform me of the duration and extent of the shock, the direction it came in, and the state of the weather before and after it was perceived. His remarks, together with the subsequent information I have obtained, great part of which I derive from your obliging communication, may serve to record this recent instance of a phænomenon to the terrors of which this country is but little exposed.

Donnington Castle, Leicestershire.

Lord Rawdon's steward was sitting with his back against an old massy stack of chimneys, which gave him a sensible shove; he at the same time saw the chairs of other persons in the room move very visibly.

Lord Rawdon's information to Sir Joseph Banks.

Lyndon, Rutland.

An earthquake was felt all over this county. What I felt, and it was the general description given of it, was, that it came with a rattling noise, something like wheels running over a pavement, and a trembling shake, which those who were leaning against a wall felt more than those who were standing, or sitting upright. It lasted a considerable time; different people estimated it from a quarter of a minute to a minute. Several thought the noise and shake came from the north or the north-east; and indeed, I think I heard more of it from the northward than the southward of us. Some thought they heard a thump, as of a heavy thing falling down; but the rattling and shaking were much more taken notice of. Several persons at first suspected it might be a ball of fire, but no such thing was seen; and many were immediately sensible it must be an earthquake, especially those who had felt one before. No material hurt was done by it; some who were standing were seen to reel, and one who was walking was thrown against a wall by it, but not hurt; a stack of wood was thrown down, and some said a chimney. I heard of it as far as Newark, in Nottinghamshire; and it was said to be felt at Biggleswade, in Bedfordshire. The season was at the conclusion of a frost, there had been a little rain, and a thaw was beginning. The barometer gradually fell from the 23d to the 26th—the thermometer was as under:

Newark.

Biggleswade.

	21st	22d	23d	24th	25th	26th	27th
morn.	16.5;	27.3;	22.3;	24.0;	34.9;	39.8;	40.2
aftern.	28.0;	32.6;	33.2;	39.0;	38.0;	45.5;	52.2

I felt the earthquake of September 30, 1750, as I did this, and heard the same rattling noise, and my seat shook under me. Many others heard and felt the same; but some described it as the falling down of a weight. September, 1750, was almost all a calm, dry, and hot season, scarce a breath of wind, or a glimpse of sun. The end was cooler; but dry and calm, as before. The 30th was cloudy and calm, but there was a gentle wind for some time after the earthquake.

Thomas Barker, Esq. in a letter to Mr. Turnor.

Nottingham. The earthquake at Nottingham, as felt by Dr. WHITE, was undulatory in both shocks; but, in the second, he perceived the direction of the shock to be a little inclined to the horizon, towards the south-east. Langar. At Langar, its noise was like that of a heavy carriage driving rapidly along. Here the shock was felt by one or more persons, but not by all in the same room. Its direction was from west to east. Derby. It was felt at Derby very slightly; and was of no great extent from north to south in these parts, as it was not felt at Harborough to the south, nor at Chesterfield to the north. Stamford. Wandesford. It was sharply felt at Stamford, and Wandesford, but not at Alconbury.

The Rev. Edw. Gregory's information to Mr. Turnor.

Belvoir Castle. The shock seemed to be perpendicular, up and down, as if part of the castle had given way; no noise was perceived but wind. *Mr. King's information to Mr. Turnor.*

Little Paunton, Lincolnshire. The morning presented a thick fog, which continued all the day; the air extremely heavy and close. About half past eight in the evening a noise came on resembling thunder, which lasted nearly half a minute, when two undulatory motions, in the quickest succession, were felt; a gust of wind, for some seconds, instantaneously succeeded, and in about five minutes it rained heavy and large drops, which continued some time. The windows, window-bells, pewter upon shelves, &c. gave ample evidence of the agitation. Its direction was from north-west to south-east. It was slightly felt at Branston, though not at Lincoln.

Branston.

Captain Pennyman, in a letter to Mr. Turnor.

Belton.

We were all much alarmed by a violent and sudden shock of an earthquake; it made the house at Belton shake, and was followed by a violent gust of wind, and a noise like thunder, which lasted about six or seven seconds. I really thought my chair was sinking into the ground, and, after I had recovered myself, I was not convinced that one of the fronts of the house, or a chimney, had not given way, till I had been in all the rooms, where I found every thing in its place. All the other servants felt it as I did, and the neighbours in the village thought their houses were going to fall upon them. It was felt at Grantham, Manthorpe, and other towns in the neighbourhood. *Chr. Driffield, in a letter to Lord Brownlow.*

Grantham.
Manthorpe.

Peterborough.

An uncommon rumbling noise was heard round my premises, 110 yards, resembling that occasioned by drawing a large garden roller slowly over the pavement. The sound did not appear like thunder over the head, but upon or near the ground. The like, in every respect, happened October 27th, 1776, about a quarter before ten at night.

Rev. Archdeacon Brown, in a letter to Mr. Turnor.

Boston.

The earthquake was felt and heard by a lady at Boston, who declared so at the time it happened, and said, she was sure it was something extraordinary, though no one else in the town observed it, or paid any attention to her, till the news of its having been felt at other places arrived.

Mr. Fydel's information to Sir Joseph Banks.

The Transactions of the Royal Society give an account of the earthquakes in the northern parts of England, in the years 1703 and 1750. That of the latter year is described as "beginning in Derbyshire, and passing off the island, through Lincolnshire and part of Cambridgeshire, its direction being from west to east."* From the preceding narrative it appears, that nearly the same tract of country was affected by the late concussion, and that it came in the same direction from west to east; circumstances which correspond with the observations of Mr. MECHEL; 1st. "That the same places are subject to returns of earthquakes at different intervals of time;"—2dly, "That earthquakes generally come to the same place from one and the same point of the compass." These, and other facts, that ingenious philosopher adduces in support of his hypothesis, that earthquakes are caused by the steam raised by waters, contained in the cavities of the earth, suddenly rushing in upon subterraneous fires; which steam, the moment it is generated, insinuates itself between the strata of the earth, and produces the undulatory motion beforementioned. It may, however, be remarked that the state of the air, before the shock, was calm, close, and gloomy, such as is described by Dr. STUKELEY as necessary to prepare the earth to receive an electrical stroke, and the circumstance of its having been partially felt in the same room may be supposed to favour that hypothesis; but yet the concussion seems not to have been so strong on the eminence at Belvoir Castle, as it was in the neighbouring vale.

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* Phil. Trans. Vol. XL. p. 722.

I have the honour to be, &c.

March 10, 1792.

EDM. TURNOR.

XV. *Experiments made with the View of decompounding Fixed Air, or Carbonic Acid. By* George Pearson, *M. D. F. R. S.*

Read May 24, 1792.

From a Paper read to the Philosophical Society of Edinburgh, in 1755, published in the second volume of the *Physical* and *Literary Essays,* Doctor Black appears to have discovered the affinities between an aëriform substance, which he called *fixed air,* and alkalies, quick-lime, and magnesia. His experiments also shewed, that many properties of these bodies depended upon the union and separation of this air. The discovery of these facts established this elastic fluid to be a peculiar species of substance.

Mr. Cavendish, Dr. Brownrigg, Dr. Priestley, Sir Torbern Bergman, Mr. Bewley, Mr. Kirwan, and other chemists, afterwards extended, very considerably, the history of fixed air. The question, whether it was a simple or compound body, was discussed; and by many persons it was believed to have been proved, that fixed air was composed of phlogiston and respirable air. But some of the principal facts, upon which this belief was founded, being afterwards demonstrated to be erroneous; and the production of fixed air being, to the apprehension of many chemists, more satisfactorily accounted for by the new principles of chemistry, this doctrine of its composition became no longer tenable. As the science of chemistry advanced, many acids were demonstrably proved to consist of a peculiar basis, and respirable air; and on the ground of analogy it was concluded, that all other acids were composed in a similar manner. Fixed air having been shewn, by Mr. BEWLEY, and by BERGMAN, to be an acid, of course its composition was considered, in the new system of chemistry, to be similar to that of all other acids. On examining facts already well ascertained, and by various experiments discovering others, no clear instance could be perceived of the *formation* of fixed air, but in those cases where charcoal was applied red hot to respirable air. Mr. LAVOISIER at last established this interesting fact, by a conclusive experiment, published in a volume of the Memoirs of the Academy of Sciences in 1781, and in his *Traité Elémentaire* in 1789, by which he demonstrated that charcoal of wood, except a minute portion of residue, as might reasonably be expected, combined with respirable air, and composed fixed air only. This is the date, therefore, of the discovery by synthesis, of the composition of fixed air; or, as I would rather call it, with Mr. LAVOISIER, carbonic acid. The proof by analysis, however, was required, to render the demonstration of the composition of this elastic fluid complete. The honour of the first analytical experiments on carbonic acid is due to Mr. TENNANT, F. R. S. who, in a paper read to this Society, in March, 1791, and contained in volume LXXXI. of the Philosophical Transactions, asserted, that charcoal and phosphoric acid were produced by applying phosphorus to red hot marble; from which he inferred, that the carbonic acid of the marble was decompounded. This decomposition, the ingenious author conceives to be effected by the united powers of affinity between phosphorus and the respirable air of the carbonic acid in the calcareous earth, and between the phosphoric acid, thus composed, and the quick-lime of the calcareous earth. That the black matter produced is really charcoal, the author has proved by adequate experiments. The inference, however, does not appear to me to be just, that the charcoal and phosphoric acid are the necessary result of the agency of the affinities, as stated by Mr. TENNANT. For the well known fact, that phosphorus cannot be produced from bone-ashes by the application of charcoal and heat, I think, only proves that the powers of affinity between respirable air and phosphorus, together with the affinity between the compound formed by their union (namely, phosphoric acid,) and quick-lime, are *not inferior* to the joint affinities between the respirable air, in the phosphoric acid, and charcoal, and between the compound of respirable air and charcoal (namely, carbonic acid) and quick-lime. Hence, from the principle referred to, it could not be concluded, that carbonic acid, combined with quick-lime, would be decompounded by phosphorus attracting its respirable air, and the phosphoric acid, thus formed, attracting the quick-lime. Experience only could determine the result of these affinities, but no proof has been given, from the examination of the mixture, after applying phosphorus to red hot marble; such as finding that carbonic acid was really decompounded, because there was a deficiency of this elastic fluid, and that the charcoal and phosphoric acid corresponded to this deficiency. Accordingly some chemists have conjectured that the small quantity of charcoal afforded in this experiment pre-existed in the phosphorus, which, it is well known, is distilled from charcoal; and others have suspected that it might have arisen from accidental impurities.

As experience, also, has taught us that phosphorated mineral alkali will not yield phosphorus by exposure to charcoal and heat, unless *plumbum corneum* be added, we cannot infer that the carbonic acid in mild mineral alkali will be decompounded by phosphorus; because, as in the case of bone-ashes and phosphorus, the joint affinities between respirable air and phosphorus, and between phosphoric acid and mineral alkali, are, by this fact, shown to be *not inferior* to the conjoined affinities between charcoal and respirable air, and between carbonic acid and that alkali. No other conclusion can be drawn with respect to the affinities exerted when charcoal is applied to phosphorated vegetable alkali; because the affinity is stronger between the phosphoric acid and vegetable alkali, than between the same acid and mineral alkali. As the attractive forces between phosphoric acid and barytes, and between that acid and magnesia are, very probably, at least equal to those between phosphoric acid and fixed alkalies, the question, whether carbonic acid united to these earths can be decomposed by phosphorus, remains to be determined by experiments. But with respect to the volatile alkali, we know, by the experience of making phosphorus from urine, that the united affinities between respirable air and phosphorus, and between phosphoric acid and volatile alkali, are *inferior* to the joint affinities between charcoal and respirable air, and carbonic acid and volatile alkali; hence, in a due degree of heat, phosphorus and mild volatile alkali are formed from phosphorated volatile alkali and charcoal, consequently carbonic acid combined with volatile alkali, cannot be decompounded by phosphorus and heat, even if the volatility of that alkali did not, apparently, render it impossible to apply the requisite degree of heat. We know so little of the degree of chemical attraction betwixt clay and phosphoric acid, that the question, whether carbonic acid united to clay will be decompounded by phosphorus; can only be answered by future experiments.

As I presume that I have made experiments which enable us to draw conclusions concerning the above cases of compound attraction, and which also shew, in several instances, that carbonic acid is decompounded, and affords respirable air, and charcoal; I think it my duty, on a subject so very interesting in the present state of chemistry, to submit them to the consideration of this Society.

Experiments with Phosphorus, applied to mild fossil Alkali.

I began with attempting to decompound carbonic acid in union with fossil alkali, in preference to the same substance combined with quick-lime, because the proportion of this elastic fluid is much greater in mild fossil alkali than in calcareous earth, because the affinity is not so strong between carbonic acid and fixed alkalies, as between carbonic acid and quick-lime, and because the mechanical separation of charcoal from alkalies, and phosphorated alkalies, may be more easily made than of charcoal from calcareous earth and phosphoric selenite. The purest fossil alkali I could procure was employed, from which I had expelled $\frac{67}{100}$ of its weight of water, but none of its carbonic acid.

Into a thick white glass tube, almost one inch wide, three feet and a half in length, coated within nine or ten inches of the open end, were introduced two hundred grains of transparent phosphorus, and eight hundred grains of the above deaquated alkali were pressed down upon them. The tube, thus charged, was then bent so that the open end might be kept conveniently plunged in quicksilver during the experiment. The coated part of the tube, containing the alkali, excepting two or three inches next to the phosphorus, was gradually heated over a portable furnace till it was red hot, and rather flexible, in which state the part containing the phosphorus was gradually drawn over the fire, and kept red hot twenty minutes. At the beginning of the experiment, quicksilver rose several inches within the tube, and when the coated part grew hot, phosphorus was sublimed into the upper and cool part of it: about twenty drops of water were condensed over the quicksilver; and two ounce measures of phlogisticated air, with a little respirable air, which had the smell of phosphorus, came over. The tube, when cold, being broken, the lower part was found to contain a loosely-cohering solid, as black as charcoal, which weighed 428 grains, and above this, a grey and white substance, partly fused, and partly in a powdery form, which, with adhering glass, weighed 358 grains. Neither in this, nor in other similar experiments, was I able to collect the whole contents of the tube, without glass which had been melted, that adhered to the alkali, on which account I could not determine accurately the total weight, independently of glass; but I was sure, from a number of trials, that it was a little less than the original weight of the alkali. The phosphorus, sublimed into the upper part of the tube, was moist from the adhering phosphoric acid: it was inflamed by slight friction, *viz.* merely on breaking the tube.

The 428 grains of black alkaline matter thus obtained, afforded, by solution in boiling hot concentrated acetous acid, a little more than 25 ounce measures of carbonic acid, under the mean pressure of the atmosphere, and of the temperature of

45°; that is, 100 grains of the black matter yielded about six ounce measures of this elastic fluid. In other similar experiments, the quantity of carbonic acid varied from four to seven ounce measures in 100 grains of this blackened alkali; except in one experiment, which afforded only three ounce measures of the acid, but the largest proportion of charcoal I ever made, namely, 12 grains.

The solution of the above 428 grains was filtered, and the residue, which was black, was lixiviated with boiling distilled water. This residue, when dried, weighed 32,4 grains; it had no taste or smell, but was an impalpably fine, intensely black, and very light powder; for it occupied a one ounce-and-a-half measure, therefore, may be estimated to have been about 22 times lighter than water. A little of this black powder, being thrown upon a red hot iron plate, ignited readily, but left a residue, which was one-fourth of its weight: this being again thrown upon the red hot iron plate, it ignited, and there remained, on cooling, a very small portion of brownish powder, which diminished to almost nothing, by being applied twice more to the iron kept red hot for several minutes. On sprinkling this black powder upon boiling nitre, it sparkled most brilliantly and detonated, leaving a colourless mass entirely soluble in water. This black powder, mixed with powdered nitre, deflagrated on exposure to heat, in a retort with the air-apparatus affixed to it, affording, over quicksilver, carbonic acid. This black matter, also, reduced the calx of lead; being mixed with tartar of vitriol, and heat being applied, *hepar sulphuris* was produced; and with phosphoric acid, phosphorus was obtained. That, therefore, these 32,4 grains were charcoal, cannot, I think, be doubted. I might add, that accidentally I found this powder, red hot, decomposed water as charcoal does.

The above filtered liquid was evaporated to one pint, and shewed signs of acidity: to it was added muriated lime till it produced no further precipitation. The dried precipitate weighed 130 grains, and was found to be phosphoric acid combined with lime; and the liquor, in which this precipitation took place, was ascertained to be muriated and acetated fossil alkali, with a little redundant acetous acid, and a small portion of phosphoric selenite.

The grey and white alkaline matter, with bits of melted glass, weighing 358 grains, as abovementioned, by solution in concentrated acetous acid, afforded 41 ounce measures of carbonic acid, and a residue upon the filter, which, when well dried, weighed 44 grains. This residuum consisted of rough, sharp-pointed, black and white particles; it was much specifically heavier than the residue of the other part of the alkaline matter abovementioned to have been examined; it deflagrated a little on being thrown upon boiling nitre, but left above $\frac{4}{5}$ of its weight of matter insoluble in water, and which I supposed was vitrified. The filtered liquor from these 358 grains of alkaline substance yielded to the precipitant muriated lime 21 grains of phosphoric selenite.

To satisfy myself still further, that carbonic acid had been destroyed in this experiment, and to form some estimate of the quantity which had disappeared, I separated it, by concentrated acetous acid, from 400 grains of mild alkali, taken from the same parcel as that was which afforded charcoal, and I found the quantity to be 104 ounce measures, or 26 ounce measures in each 100 grains of mild alkali.

To afford a still more decisive proof that carbonic acid had not been combined, or escaped, in this experiment, but had been destroyed, I exposed some of the same parcel of alkali which had afforded charcoal to the same degree of heat, in tubes, under similar circumstances to those in the above experiment; and I found that no carbonic acid, but a little water, came over into the air-apparatus; that the total weight of the alkali was diminished, but that a given weight of it, after the experiment, afforded rather more carbonic acid, by solution in acetous acid, than an equal weight of the same parcel of alkali not thus subjected to heat. This diminution of weight of alkali, and greater proportion of carbonic acid, I impute to the water visibly separated in the glass tubes, and, perhaps, also absorbed in the earthen ones. Accident afforded a still more decisive proof of the decomposition of carbonic acid. In the beginning of the experiment, the tubes sometimes cracked about four or five inches from the part containing the phosphorus: on cooling, I found, in the part below the crack, black alkaline matter, which yielded much less carbonic acid than the same weight of alkali before the experiment; whereas the alkali above the crack was white, and contained the same quantity of this elastic fluid that it did before it was exposed to heat.

In the experiment above particularly described, it appears that in one part of the alkali there was a deficiency of twenty ounce measures of carbonic acid *per cent.* of alkali; but a production of rather more than eight grains of charcoal, and of as much phosphoric acid as formed about thirty grains of phosphoric selenite; the composition of which may be estimated to be, of phosphorus, five grains; respirable air, ten grains; and quick-lime, fifteen grains. Now, as it has been demonstrated by M. LAVOISIER, that charcoal, either totally, or a minute proportion excepted, combines with respirable air, and forms carbonic acid; and other familiarly known, though less accurate, experiments, show that carbonic acid is formed whenever charcoal and respirable air are applied to each other in a due degree of heat; and as there are no other sources perceivable of respirable air and charcoal in this experiment, it seems to prove decisively that they are derived from the carbonic acid, which is decompounded by the superior affinities between phosphorus and respirable air, and phosphoric acid and alkali, to those between respirable air and charcoal, and carbonic acid and alkali. An additional proof of the reality of this decomposition is afforded by the examination of the 358 grains of white and grey alkaline matter, of the same experiment, which afforded much more carbonic acid, and much less charcoal and phosphoric acid. I am very fully aware that the proportions of respirable air and charcoal, produced in this experiment, do not correspond to the proportions of them we should have expected, consistently with the synthetical experiments concerning carbonic acid. The variation is especially great with respect to respirable air, of which there should have been eighteen grains instead of five, to combine with the whole of the charcoal, but, from the nature of the experiment, we cannot even approximate to the truth with respect to the real quantity of respirable air produced; for the phosphorus which sublimed probably carried off a little of this air, some of the phosphoric acid thus formed fused along with alkali and glass, and some phosphoric selenite remained dissolved in the liquid. Supposing the whole of the charcoal formed in this experiment to be united to respirable air, the quantity of carbonic acid composed may be calculated to be 104 grains; for 32 grains of charcoal combined with 72 of respirable air compose 104 grains of carbonic acid, or 70 ounce measures; to which must be added the 25 ounce measures of undecompounded carbonic acid separated. Then the quantity of this elastic fluid, calculated to be decompounded, and remaining united in about 400 grains of mild fossil alkali, is 95 ounce measures, and the quantity of it actually found to exist in an equal weight of alkali, was about .112 ounce measures: therefore the quantity of charcoal produced does not differ very considerably from that calculated to be contained in the carbonic acid decompounded. But future experiments must determine whether there is a like coincidence with respect to the other supposed constituent of carbonic acid, namely, respirable air.

I deem it unnecessary to relate a number of experiments which I have made, the result of which was similar to the preceding one; but it may be proper to mention, that in every instance, the proportions of phosphoric acid and charcoal were inversely as the quantity of carbonic acid remaining in the alkali; and that the quantities of these two products diminished as the quantity abovementioned of phosphorus was diminished; accordingly, the alkali most exposed to the phosphorus contained the greatest proportion of charcoal.

I made this experiment several times with alkali, which contained a good deal of water, and then I obtained a large quantity of air, which smelt of phosphorus, but did not explode on contact with atmospheric air; it contained no carbonic acid, nor phlogisticated air, excepting a few ounce measures in the first jar that came over, but it exploded loudly when mixed with an equal bulk of dephlogisticated air, on applying a lighted wax taper. A charge of 95 grains of phosphorus, and 540 grains of the above alkali, afforded 206 ounce measures of this inflammable air, which was of the same quality whether it was received over water or quicksilver. This air, I apprehend, was produced by the decomposition of the water in the alkali, in consequence of the superior affinity between phosphorus and respirable air, to the affinity betwixt respirable and inflammable air. Therefore when moist alkali is used, *cæt. par.* more phosphoric acid will be formed than when dry alkali is employed; and in calculating the quantity of respirable air formed, regard must be paid to the decomposition of water. It appears also, that it requires less heat to decompound water by phosphorus, than to disunite carbonic acid from fixed alkali.

In these experiments I frequently used thick white glass tubes, and applied heat for a long time, to the degree of rendering them flexible: when cold, I found the internal surface in contact with the black alkaline matter full of cells, or small cavities, and rough, to which small grains of lead adhered, consequently the respirable air of the calx of this metal in the glass had been attracted, and reduction effected. This reduction might be produced by three substances here present, namely, phosphorus, inflammable air, and charcoal; but I impute it to the charcoal; 1st. because I obtained no particles of lead by passing phosphorus through a tube filled with powdered white glass, heated to the degree of rendering the tube soft, although on cooling, I found the internal surface of the tube was turned black, which colour could not be removed by rubbing, or by acids. This appearance I cannot explain. 2dly. This reduction is effected when there is no water present, at least when no inflammable air is extricated. 3dly. The greatest quantity of regulus of lead was obtained in those parts of the alkaline matter which contained the smallest quantity of charcoal, and therefore, I conceive, the charcoal, formed in those parts, had united to the air of the calx after the phosphorus had been driven through the alkali, so that the carbonic acid thus composed could not be decompounded, but was combined with the alkali, which was always redundant. In calculating the proportion of carbonic acid decomposed, it will be necessary to consider the reduction which here takes place.

If the air-apparatus be not affixed to the tube, containing a charge of phosphorus and alkali, charcoal and respirable air will be formed; but the phosphorus will take fire at the open end of the tube, and burn with splendour, as in dephlogisticated air.

Porcelain, or well glazed WEDGWOOD tubes, answer in these experiments better than glass ones, the insides of which are apt to melt; but unglazed vessels allow the phosphorus to pass through their pores, though part of the carbonic acid may be decompounded.

The heat applied must be greater than the white glass now made can endure without melting; for, I passed phosphorus through a tube containing mild fossil alkali, heated so that it appeared red hot in the dark, and no charcoal was formed, though the inside of the tube was blackened.

Experiments with Phosphorus applied to mild vegetable Alkali, calcareous Earth, Barytes, Magnesia Alba, and Clay.

Similar experiments to the preceding, made with mild alkali of tartar, in place of fossil alkali, afforded, apparently, as much charcoal, and which was easily obtained, but as the phænomena were similar, and as I have not ascertained with any tolerable precision the proportion of the carbonic acid decompounded, and of the products, it is unnecessary to give any further account of them.

By the like experiments, I endeavoured to decompound the carbonic acid in calcareous, barytic, magnesian, and argillaceous earths. The matter remaining in the tubes, after exposure to heat, was blackish, and grey, seemingly from charcoal being formed, though in much smaller quantity than in the preceding experiments with fixed alkalies. For the reasons above given, I omit the particulars of these experiments on earths.

It appears to me, that the above experiments justify the inference that the joint affinities between respirable air and phosphorus, and between phosphoric acid and mineral alkali, are superior to the affinity between the whole, or at least part, of the respirable air of carbonic acid and charcoal, cooperating with the affinity between that acid and the same alkali. And, although I have not ascertained the facts with equal satisfaction, the experiments already made seem to warrant the conclusion, that the order of the affinities is such, that carbonic acid united to vegetable alkali, lime, barytes, magnesia, and clay, will be decomposed by phosphorus in a due degree of heat. With respect to carbonic acid combined with volatile alkali, as might be expected, I could not decompose it, although I transmitted boiling hot phosphorus through a very long tube, containing mild volatile alkali.

Experiments with Phosphorus applied to Quick-lime, and caustic fixed Alkalies.

I need not explain that these experiments must confirm or invalidate the conclusion above drawn, that carbonic acid was decomposed by phosphorus applied to mild alkalies, and earths which contain this elastic fluid.

As the quick-lime which can be procured in London must contain both water and carbonic acid, I exposed a quantity of this earth 48 hours to the fire of a reverberatory furnace, by which it was contracted to half its former bulk, and was diminished in its weight; it was, however, still soluble in acids, and afforded no carbonic acid. In the manner above described, I exposed 240 grains of it, with 60 grains of phosphorus, to heat in a coated glass tube. On breaking the tube, when cold, I found at the bottom about 30 grains of blackish and white powder; and above that, to the extent of four or five inches, was a rose-coloured powder, which by its contact with air soon became of a reddish brown colour; above this was the quick-lime, scarcely altered in its colour, but it had, like the rest of the powder in the tube, an alliaceous smell. On tasting a little of this reddish powder, I was surprised by its exploding upon my tongue. I threw a few grains of it into several ounces of cold water, it did not seemingly dissolve, or turn black, but in a few minutes emitted air-bubbles, which rose to the surface of the water, and then burst and exploded, producing a white circular cloud, which in ascending expanded gradually, till it burst in the air. It continued to emit these bubbles from time to time, during an hour, and then left a grey sediment, which was phosphoric selenite and lime, and the water tasted strongly of lime. The same powder, in hot water, exploded more rapidly and loudly than in cold, but not so violently as the phosphoric air obtained by boiling phosphorus in a lixivium of caustic fixed alkali. By putting this powder into an inverted jar of water, I collected a quantity of the air which it produced; it had the properties of the phosphoric air already mentioned, and, amongst others, by standing over water a day or two, it became no longer spontaneously inflammable, but appeared to have deposited phosphorus upon the water and sides of the vessel, and exploded upon applying to it a lighted wax taper. This powder, therefore, I apprehend, consists of phosphorus and lime united by heat; it readily decomposes cold water, and then the inflammable air disengaged unites with, or rather suspends, a portion of phosphorus, and forms phosphoric air. The phosphorus thus suspended by standing is deposited, and inflammable air alone remains; the other constituent of water, respirable air, unites to another portion of phosphorus, and composes phosphoric acid, which combines with lime, and forms phosphoric selenite. This compound of lime and phosphorus, which some of my chemical friends have called *fulminating hepar of phosphorus*,* may be used to obtain phosphoric air with much more ease than by the usual method. †

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* In the new system of chemistry it will be called *phosphur of lime*.

† Dr. INGENHOUSZ has devised some surprising and beautiful experiments with this substance.

This experiment seems decisive, that the charcoal afforded in the former ones was derived from carbonic acid.

My next experiment was with caustic alkali and phosphorus. The caustic vegetable alkali I employed was blackish, partly from a very small quantity of calx of iron, and partly, I think, from other causes which I do not understand; and I was not able to prepare myself, or obtain from others, fixed caustic alkali in a solid form which was colourless. It also always contained a small quantity of carbonic acid. I introduced into a glass tube 50 grains of phosphorus, and 150 of pulverized caustic vegetable alkali, previously found to contain three ounce measures of carbonic acid in each 100 grains. This charge was exposed to heat, as in the former experiments. On breaking the tube, when cold, the alkaline matter was blacker than before: a little of it thrown into hot water emitted bubbles of phosphoric air, but not in cold water: in rubbing off this alkali from the sides of the tube some pieces of it took fire. I dissolved as much as I could of this black alkaline matter, by pouring boiling water upon it on a filter: a greenish lixivium passed through first, then a less coloured alkaline liquor; and last of all, limpid water. A residue left upon the filter being dried, weighed ten grains; it was a blackish brown, impalpable powder, at least five times specifically heavier than the charcoal obtained in the preceding experiments.

(a) Six grains of this residue upon a thin plate of iron, heated over a candle, burnt with a green and blue flame, emitting a somewhat arsenical odour, and it did not remain ignited after the flame ceased. A coal-like matter was left, which weighed three grains.

(b) These three grains *(a)* being placed upon an iron plate, red hot, again emitted a little green and blue flame, with the like, but a weaker smell than before; the substance remaining continued ignited but a few seconds of time, though the iron was red hot much longer. The residuum, which was black, weighed two grains and a half.

(c) The residuum *(b)* was exposed in a silver spoon red hot; it soon ignited, and sparkled; but though this heat was applied six minutes, on cooling, a blackish matter remained, which weighed 1,3 grains.

(d) The 1,3 grains of residue *(c)*, under the flame applied with the blow-pipe, gave some indications of fusion, yet it remained black: but

(e) Being thrown into boiling nitre, a slight detonation ensued; this salt was not coloured by it, and it was dissolved in water, leaving scarcely a visible quantity of matter upon the filter.

I think I may safely conclude that but a small part of these ten grains of residue was charcoal: and as the proportion is so much smaller than has been shown to be afforded by an equal weight of alkali saturated with carbonic acid, this experiment confirms the conclusion, that the charcoal produced in the preceding experiments is from the decomposition of that elastic fluid. The small quantity of charcoal in the above ten grains of residuum was, perhaps, intimately mixed with phosphorus and alkali; but more experiments are required to determine satisfactorily the nature of this compound.

To corroborate the inference concerning the source of the charcoal above described, I add, that not a grain of it was produced by applying phosphorus to vegetable alkali and fossil alkali saturated with vitriolic and marine acid.

The resemblance betwixt phosphorus and sulphur, induced me to consider whether carbonic acid, combined with alkalies and earths, might not be decompounded by sulphur. Experience, however, did not favour the supposition of a decomposition in these instances; for it is well known that hepar may be formed by applying charcoal to tartar of vitriol, GLAUBER's salt, vitriolic selenite, and ponderous spar: and therefore that the affinity between charcoal and respirable air is superior to the joint affinities between respirable air and sulphur, and between vitriolic acid and fixed alkalies, lime, and barytes; consequently, if sulphur be applied to carbonic acid, united to these alkalies and earths, the affinity between sulphur and respirable air cannot disengage charcoal from the carbonic acid in mild alkalies and absorbent earths. This conclusion would, however, only be just, provided no other agents interfered; and as we cannot be absolutely certain that they could not, I repeated the above experiments with sulphur instead of phosphorus; by which I produced a blackish powder, that had the properties of reducing calx of lead, and changing vitriolic salts into hepars: but as it did not burn on red hot iron, and deflagrate with nitre, I cannot pronounce it to be charcoal; thinking it most prudent to reserve this matter for future examination.

Leicester-Square, September 20, 1792.

P.S. In consequence of some observations published in the *Annales de Chimie*, Juin, 1792, Tome XIII. by Mr. FOURCROY, it is thought proper to add, that although the above paper was not read till May last, it was presented to the Society in March preceding; and the experiments were made during the author's autumnal course of lectures in 1791. In these experiments he was assisted by Mr. EGGINTON, of Queen's College, Cambridge, who attended that course. The products were shown, and the experiments mentioned to several Members of the Royal Society the beginning of last winter, particularly to the President, who honoured the author with his attendance during several of the processes.

The substance produced by M. RAYMOND, referred to in the *Annales de Chimie,* is a *humid* combination of phosphorus and lime, and does not decompound cold water; it is therefore a very different composition from the *phosphur of lime* described in the above paper.

XVI. *Observations on the Atmospheres of Venus and the Moon, their respective Densities, perpendicular Heights, and the Twilight occasioned by them.* *By* John Jerome Schroeter, *Esq. of* Lilienthal, *in the Dutchy of* Bremen. *Translated from the* German.

Read May 24, 1792.

On the Atmosphere of Venus.

Although the evidence afforded us by the most recent observations and discoveries, not only of the existence, but also of the various and singular properties of the atmospheres of Saturn, Jupiter, Mars, and the Moon, be in a manner incontrovertible; yet so inconclusive are the few observations hitherto made on the atmosphere of Venus, that several of the greatest astronomers* have lately thought themselves authorized to doubt its very existence.

The following inquiry may hence prove the more acceptable to those who favour the investigations of nature, not only as it evinces, in my opinion, the existence of such an atmosphere, but also as it points out several inferences concerning its nature and properties, which to me appear new and interesting.

What convinced me, twelve years ago, when I first began to observe Venus with a good three-feet achromatic telescope, that it actually has an atmosphere of no small extent, was the striking diminution of light which I noticed on the planet in its various phases from its exterior limb towards the interior edge of its illuminated surface, and especially near the latter: and this appearance it was which induced me to make farther observations on the subject, especially as I found that the phænomenon recurred as often as I looked at the planet with an HERSCHELLEAN four and seven-feet reflector, armed with the higher magnifying powers.

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* See *Astronomie de* M. de la Lande, §. 2272.

The great number of observations I have now made on this object, for a series of years, being upon the whole very similar in their nature and results, it would no doubt be not only tedious, but also superfluous, to describe them here at length: but the following general remarks it may be necessary to premise, in order to obviate all misapprehension, and the false conclusions that might be deduced from hasty and inaccurate observations.

In fig. 1. Tab. VI. which I have annexed for the sake of greater perspicuity, the light appears strongest at the outward limb *a*, *b*, *c*, from whence it decreases gradually, and in a regular progression towards the interior edge or terminator, and this not only towards its middle *d*, *e*, but also near the two cusps *f g*, *h i*, the light becoming so dim immediately at this border *g*, *f*, *d*, *e*, *h*, *i*, that it commonly loses itself in a faint bluish grey, forming a very indefinite ragged margin, scarce discernible with the best telescopes, and which, in several of the phases, resembles the interior uneven border of the moon, as it appears to the naked eye, or to a power magnifying from 1 to 4 times. The following circumstances must necessarily be attended to respecting these observations.

1. This diminution of light naturally shews itself between the greater eastern and western elongations and the inferior conjunction, when Venus appears about half full, or falcated, but still of sufficient breadth.

2. But this diminution is not equally sensible in all the phases of the planet, but sometimes much more so than at others. These appearances depend greatly on the clearness of our atmosphere, on the telescope giving a very distinct, soft, mellow image, and on the eye being properly prepared for such observations.

3. When, in the more falcated phases, we wish to observe distinctly and accurately this diminution of light at the farther extremities of the cusps, it is necessary to advert that, whilst about the middle of the terminating border between *d*, *e*, where the luminous part has its greatest breadth, this diminution, if seen under favourable circumstances, is too perceptible to leave the least doubt or suspicion, the points of the cusps, especially when the weather and other collateral circumstances are not favourable, appear, indeed, somewhat fainter, but yet almost as bright as the outward limb near *b*. As the diminution of light, whether the cause proceed from its own nature, or from that of the atmosphere, must be in the same proportion at the cusps as at the middle of the terminator, I could not but be puzzled at this appearance; but the multitude of observations I have since made on the points of the cusps, sometimes when they were tolerably bright, and at others when they appeared as faint as the other parts of the said border, at length cleared up my doubts, and led me to the true cause of this variable appearance, which, to prevent similar perplexities in others, I shall here state at length.

Considering the immense height of the mountains, and the great inequalities on the surface of Venus,* it is natural to suppose that, as at the times of its greatest elongations, one cusp frequently appears pointed and the other blunt, owing to the shadow of some mountain darkening the extremity of the latter, the same appearance may often take place in the falcated phase of the planet. But the cusp whose extremity is covered by a shadow, will, in this case, so far from appearing blunt, always exhibit a pointed appearance; it being in that aspect too sharp for us to distinguish accurately its true form. Should, for instance, the point of the cusp $\alpha\ \beta\ \gamma$, fig. 2, be immersed in a shadow from α to β, this point, in such a case, though it may be somewhat blunted from α to γ, will nevertheless appear sharp, because its whole breadth does often not exceed $\frac{1}{4}$ of a second, whence its true form must needs escape our observation. We do not, in this instance, see the very faintly illuminated point $\alpha\ \beta$, nor yet the real luminous edge $\gamma\ \beta$, but only its prominent and brighter part $\alpha\ \gamma$; a deception this, which cannot take place in the other parts of the terminator, where indeed the same causes, *viz.* the shadows of mountains, will, no doubt, at times occasion an uneven, ragged appearance, but cannot materially affect the very faint light of the whole.

It is, moreover, evident, that even when the extremities of the cusps are, as in fig. 1, not darkened by shadows, the faint luminous points *f g*, *h i*, are not always visible, but can only be seen with certainty at such times when the air is very clear and calm, and a good telescope with a large aperture, and high magnifying power, is used in the observation. And it follows that, except under those circumstances, the parts of the cusps *a f*, *c h*, must, on account of their great sharpness, be the apparent and sufficiently luminous extremities; since, according to well known principles, the exterior points *a*, and *c* will have as much light as the point *k*, near the middle of the crescent.

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* The perpendicular height of these mountains, as I have already shewn in a former work, is in proportion to the diameter of the planet, at least as great as the height of the mountains in the moon, in proportion to its much shorter diameter; this fact I have had frequent opportunities to ascertain. See Selenotop. Fragm. §. 522.

But in a clear and calm atmosphere, and with a high magnifying power, it is truly pleasing to see, after the eye is accustomed to it, how the whole of the terminating border, even to the farther extremities of the cusps, vanishes gradually, and becomes at last so faint, that in the day time, and where there are any inequalities, it insensibly loses itself in the colour of the sky. Such striking diminutions of light have I seen repeatedly with my four-feet reflector, with a power magnifying 280 times, and my seven-feet reflector, with a 370 magnifying power; and particularly on the 20th of November, 1791, when with a power of 161, I saw the light of the terminator dwindle away, and appearing, for a breadth of about 1 or $1\frac{1}{2}$ seconds, almost as grey as the ash-coloured spots on the moon.

Those who are at all acquainted with the theory of light, need hardly be reminded that on an illuminated spherical surface of a planet, the light will ever appear fainter towards its border, in proportion as the angle between the incident ray from the sun and the said surface becomes smaller.*

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* It may equally be superfluous to illustrate how, since the sun appears on the planet Venus under a diameter of about 44′, its luminous border must have a no inconsiderable penumbra. It appears, however, from calculation, that this whole pe-

But what here claims our particular notice, is the singular circumstance that, except in the planet Mercury, concerning which I have not yet a sufficient number of observations, a similar diminution of light is not observed in so sensible a degree on any of the other planets of our solar system, our earth only excepted.

Mars, Jupiter, and Saturn, cannot, indeed, on account of their great distance, exhibit on our globe the variable phases of a half, or smaller portion, of an illuminated hemisphere, whence no fair arguments can be derived from those instances: but the comparative appearances of the moon, in this respect, will be thought the more singular if carefully attended to, the marginal diminution of light on this satellite, which, however, like Venus, is a sphere illuminated by the sun, not being nearly so perceptible and evident, as that above described. Of this we may fully persuade ourselves, by comparing the appearances of the terminating borders of the moon in its falcated phases or quadratures, with the same borders on Venus at the same periodical aspects. Should this striking difference not be reconcileable upon our established optical principles, nothing will remain but the analogy, that, as the density of our atmosphere checks the sun-beams the more, the longer they proceed therein in a direction which, after the rise or before the setting of the sun, carries them over a certain track of land; and as such a track, on which the sun at its rising and

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numbra, which, according to the greatest apparent diameter of Venus, extends from 59 to 60″, measures in the direction perpendicular to the line of the cusps only 0,36″, or $\frac{1}{3}$ of a second of a degree; and that where this penumbra is observable in its exterior darker parts, its breadth is not above $\frac{1}{10}$ of a second, whence it cannot be visible throughout.

setting shines from the horizon, is but feebly illuminated; so also is Venus circumstanced with regard to the light it receives from the sun. To compare with some accuracy the intensity of light at the terminating borders on our earth, relatively to its full perpendicular illumination, with the same phænomenon on Venus, may, for want of opportunities to observe both planets at once from a proper distance, as may be done with regard to Venus and the moon, not be very feasible; but this, I think, I may with great confidence infer from my long series of observations, that Venus has an atmosphere in some respects similar to that of our earth, and which must far exceed that of the moon in its density, or power to weaken the rays of the sun.

Thus far had I proceeded in my observations, when the heavens favoured me with the following ones, which may prove the more interesting, as they not only confirm those hitherto made, but also lead to some farther inferences concerning the atmosphere of Venus.

Among all the favourable circumstances for observing the planet Venus, none could be more so than those I had in the months of March and April, but especially from the 9th to the 16th of March, 1790, when, besides the almost constant and unusual serenity of the sky, the planet, which was then in Aries, at 7° and 8° N. declination, was so high above the horizon, that, notwithstanding its approaching inferior conjunction on the 18th of March, at 4 P. M. I could still view it on the 16th, and should certainly have observed it during the conjunction, had not the weather become hazy on the 17th.

Under these very fortunate circumstances, I fell accidentally (not having, after ten years of constant attention to Venus, been able to devise any new mode of observing) on the 9th, 10th, 11th, and 12th of March, upon an observation, which I repeatedly confirmed, and which, on account of its singularity, and the light it will probably throw on the physical constitution of this planet, will certainly be ever thought important; especially as it may not in many years be repeated under so favourable a combination of incidents.

On the 9th of March, 1790, immediately after sun-set, and till $6^h\ 45'$, I saw Venus with a seven-feet reflector, magnifying 74,95, and 161 times, very distinctly, and uncommonly splendid. The southern cusp did not appear precisely of its usual circular form, but rather as is represented at *a*, fig. 3, inflected in the shape of a hook beyond the luminous semicircle into the dark hemisphere of the planet. This, however, after my former observations,* was not new to me: but a far more striking phænomenon, which I had never seen before, excited my admiration, and particular attention. The northern cusp was terminated at *b*, in the same narrow tapering manner as the southern, but did not extend in its bright luminous state into the dark hemisphere. From its point, however, the light of which, though gradually fading, was yet of sufficient brightness, a streak of glimmering bluish light proceeded into the dark hemisphere, from *b* to *c*, which though intermittent as to intensity, was yet permanent as to duration, and although very faint, could yet be plainly seen with both the abovementioned magnifying powers. Like the luminous line then seen on Saturn, its light seemed to twinkle in various detached points, and appeared throughout not only very faint, when compared with the light at the point of the cusp, but also of a very peculiar kind of faintness, verging towards a pale greyish hue.

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* See Selen. Fragm. §. 523.

The limb of the planet at this small part of its dark moiety, appeared with as faint a light, and, compared with the extremity of the southern cusp, as pale as the dark limb of the moon three days before and after the new moon, when it is faintly illuminated by the reflected rays from the earth: and it appeared to me, that towards the farther extremity *c*, where it was actually inflected, according to the circular limb of the dark hemisphere, its light vanished into a pale bluish tint, in the same manner as the more vivid light of the luminous hemisphere dwindles away towards the terminating border and the extremities of the cusps. The 3d fig. represents this striking phænomenon only in its projection, it being impossible to give a just representation of it in a drawing. Surprised at this new and singular appearance, I pointed it out to Mr. TISCHBEIN, an ingenious artist, who was then with me engraving the plates for my Selenotopographic Fragments, and desired him to attend particularly to all its circumstances.

He found and described the whole just as I had seen it. I made the drawing, fig. 3, under his eye; and we viewed the object together very attentively for about an hour, when the planet descended too low to be distinctly seen.

The apparent diameter of Venus was, by means of the projecting table, and the mean of several observations, all of which agreed to within 1″, found to measure 59″; but the greatest breadth of the illuminated part did not exceed 2,6″.

On the following evening, the air being as calm and serene as the preceding one, I observed the planet from 6^h to 6^h 40′, but on account of some necessary alterations in the apparatus of the seven-feet reflector, I could use only the four-feet instrument, with powers magnifying 134 and 70 times. The southern cusp had its luminous prolongation, but not quite so distinct as the preceding night; but what was more remarkable, each cusp, but chiefly the northern one, had now most evidently a faint tapering prolongation, of a bluish grey cast, which, gradually fading, extended along the dark hemisphere, so that the luminous part of the limb was considerably more than a semicircle.

On the next night, being the 11th of March, when the seven-feet reflector was again fit for use, I found Venus before sun-set, with a power magnifying 95 times. At six o'clock, I saw distinctly the southern point terminating in a luminous streak; which now, as in the evening of the 9th, was longer and narrower than the bright termination of the northern cusp. The apparent diameter of Venus measured 60″; but the greatest breadth of the illuminated part could not be ascertained on account of some undulations in the air, occasioned by rising vapours: the light, however, sparkled through this tremulous medium, similar to the flash in a silver furnace; and comparing what I could distinguish with the table of projections, this breadth I found could not well exceed 2″. I also saw most distinctly the faint bluish luminous streak, sensibly extending in an inflected direction beyond the bright semicircle, and continued observing it till 6^h 30″, when, on account of the descent of the planet, I ceased my observation. The powers I used magnified 95 and 74 times.

No doubt can hence be entertained of the reality of this memorable phænomenon; but for reasons which I shall give hereafter, I was very solicitous to be able to measure the projection of this glimmering strip of pale light, and to ascertain whether in fact it only belongs to the northern cusp.

The very next, or the fourth evening, gave me a favourable opportunity for this purpose, and this was the more fortunate, as, on account of the hazy weather that immediately succeeded, of the increased twilight into which the planet advanced, and of its decreasing declination, this was the last evening in which such an observation could be made with some precision. At six o'clock, the atmosphere being uncommonly clear, I looked at Venus with the seven-feet reflector, magnifying 95 and 74 times. It appeared very distinct, and I ascertained, beyond the possibility of doubt, that the southern cusp projected somewhat into the dark hemisphere, and that from the point of the northern one, the very faint narrow streak of pale bluish light, intermittent in intensity on account of its faintness, but yet permanent as to duration, extended several degrees along the limb of the dark hemisphere of the planet. I strained all my visual powers, but could actually see this appearance only at the northern point. As the planet descended towards the horizon, the light of the very sharp southern, as well as that of the bright part of the northern point began to glimmer with gradual diminution, the latter more faintly than the former; but not the least appearance of a pale bluish prolongation could be traced at the southern cusp.

I had immediate recourse to the projection machine, and though I found it very difficult to measure, with the greatest accuracy, the very faint light at the northern, as well as the bright prolongation at the southern cusp, I succeeded, however, to ascertain

1. That the faint streak at the northern cusp, as represented in its projection, fig. 3. extended at least 8″ of a degree along the limb of the dark hemisphere.

2. That the prolongation of the southern cusp measured likewise full 8″, its inflected hooked form appearing now very distinctly: and,

3. That the apparent diameter of Venus subtended an angle of between 59 and 60″.

Had it not been for these very favourable observations from the 9th to the 12th of March, 1790, which evinced, beyond a possibility of doubt, that the illuminated limb of Venus actually exceeds the semicircle, I should, perhaps, never have arrived at the important cosmological discovery it led to; the days preceding them being too hazy, and those following, though somewhat clearer, yet too near the inferior conjunction of the planet to admit of fair observations. On the 17th, and on the 18th, when the conjunction took place at 4 P. M. the sky was cloudy. From the 21st to the 25th, as also on the 30th of March, the 1st, 7th, 8th, and 21st of April, the mornings about five o'clock were indeed sufficiently clear, and it may easily be imagined, that, considering the importance of the discovery, I did not fail to avail myself of them; but the northern declination of the planet had now decreased too much, and on the other hand, the twilight in our latitude was become too great to see distinctly and with certainty what I had observed before. I had no doubt, at times, of the prolongation of either cusp into the dark hemisphere; but circumstanced as the objects were, I was much upon my guard against visual deceptions. My journal, however, supplies me with the following appearances:

1. March 23d, 5^h 10′, $4\frac{1}{2}$ days after the conjunction, a faint light appeared, at times, playing along the dark limb from the southern cusp. The arc appeared hooked and irregular.

2. March the 24th, 5^h. I saw the exterior limb of the illuminated part manifestly much more convex at *a*, fig. 4. than at *b*. I also actually saw, at present, near *d*, a grey, faint, twinkling light, proceeding from the southern cusp in an inflected direction. Something of a similar appearance was likewise seen at the northern cusp *c*; but this was still fainter, and somewhat less inflected. The bright extremities of the cusps were very sharp, and similar in point of illumination.

3. March 25th, 5^h 15′. The bright hooked prolongations extending from both cusps, were not so perceptible as on the preceding days; but I was now the more struck on seeing, under the present circumstances, manifest traces of the same faint ash-coloured inflected streaks extending into the dark hemisphere, which I had beheld on the 10th of March, and which now proceeded from the extremities of both cusps, but more perceptibly so from the southern. See fig. 5.

4. March 30th, $11\frac{1}{2}$ days after the conjunction. I now saw no more of these appearances than I had done on the 9th and the preceding days of this month, particularly the 2d and 5th, when nothing of this phænomenon could be perceived.

As these observations, made after the conjunction, fully confirm those from the 9th to the 12th of March, I shall now proceed to state the inferences that appear to me evidently to result from them.

As there can now remain no doubt of the appearance of the pale ash-coloured streak of light, *b*, *c*, fig. 3. extending along the limb of the dark hemisphere of Venus; and as this planet cannot be said, like the moon, to receive some light upon its dark side from our earth, or any other heavenly body, it follows that this light must either proceed immediately from the sun, which, as I have frequently observed concerning the high mountains Leibnitz and Doerfel in the moon,* throws its rays directly on the tops of very lofty ridges of mountains; or else that it is a light which partly illuminates the atmosphere of Venus, and partly, being reflected by this atmosphere, marks out by a faint glimmer the limb of the dark hemisphere of the planet, in the same manner as our morning and evening twilight acts upon ours.

All our present observations militate against the supposition of this phænomenon being the effect of light immediately proceeding from the sun; for,

1. This light does not appear, as on the mountains Leibnitz and Doerfel in the moon, in single, detached, and distant points; but as a continued streak of light, proceeding from the extremities of the cusps, and continuing along the limb of the dark hemisphere to a distance of about 8″, or, in proportion to the apparent diameter of the planet, no less than 15° 19′ of its circumference. This light also, compared with the bright part of the phase, is not unlike the comparatively pale limb of the dark part of the moon before and after its conjunction.

2. Were this the light of the illuminated summits of a chain of mountains, it would not appear so even, regularly connected, and spherical, as we behold it. But what removes all manner of doubt is,

3. The very different, extremely faint, bluish ash-coloured appearance of this glimmering light, which forms as great a

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* See Selen. Frag. § 75. and Tab. IV. fig. 6 and 8.

contrast with the whitish more vivid light which is seen immediately on the cusps, as the ash-coloured light reflected from our earth on the dark limb of the moon does, when compared with the solar light on its phase. This pale light in the dark hemisphere, it must be owned, faded away towards its termination, in the same manner as the solar light did at the edge of the bright phase: but had this faint streak, like the more vivid light, been an immediate emanation from the sun, the gradual diminution would have been throughout progressive in a continued proportion; and the light in the dark part, immediately contiguous to the points of the cusps, must have had nearly the same degree of brightness as the points themselves, which was by no means the case.

Every circumstance, therefore, seems to evince that this phænomenon is occasioned by a light reflected by the atmosphere of Venus into the dark hemisphere of the planet, being in some measure the light of the atmosphere itself, when illuminated by the rays of the sun, or, in fact, a real twilight. But this will appear still more manifest when

4. We compare, according to the abovementioned observations, the alternate relative appearances of the cusps of Venus reciprocally with each other. On the 9th and 12th of March, 1790, when the southern cusp extended, not in the true spherical curve of the limb of the planet, but in a somewhat hooked direction, into the dark hemisphere, the pale bluish ash-coloured streak appeared only at the point of the northern cusp, from whence it proceeded, in a true spherical curve, along the dark limb of the planet. On the 10th of March, on the other hand, when the southern cusp did not penetrate so far into the dark hemisphere, the pale streak was perceived at both

points, though somewhat more sensibly at the northern than at the southern; and such also were the appearances after the inferior conjunction. These appearances will be thus explained by the effects of a twilight. The bright prolongation of the southern cusp, as it was seen on the 10th and 12th of March, *a*, fig. 3, must be ascribed to the solar light illuminating a high ridge of mountains situated at this region, whence it appears why this prolongation might not be strictly spherical. The twilight that must have existed at this part, would naturally be eclipsed by the much greater brightness of the light immediately derived from the sun, in the same manner as, on our earth, mountains that face the rising or setting sun, are known to darken the twilight that ought faintly to illuminate the regions situated immediately behind them. Were this not the case, there is no doubt but that a true spherical arch of the limb of the dark hemisphere would have appeared faintly illuminated: and such did we see was the effect of this twilight produced at both cusps when the bright prolongation was not considerable, the appearance being most decisive at that cusp where there was least of this prolongation.

Should this pale streak of bluish ash-coloured light actually be the effect of a twilight, it will not only prove the existence of an atmosphere, but we shall also, from the length of the streak, as measured on the 12th of March, 1790, be able to draw some inferences concerning its density, and real extent on the surface of the planet.

According to that observation, the apparent diameter of the planet measured 60″, and the visible stretch of the twilight, as far as it could be seen with a power of 95, applied to a seven-feet reflector, was estimated at 8″, or eight of those parts of which 60 go to the diameter. This gives a circumference of 188,4, and the degree will be equal to 0,52 of these parts: and taking those 8″ as a chord, the arc, *b c*, fig. 3, over which the twilight extends, will measure 15° 19′; this being the portion of a great circle over which, in circumstances as favourable as those I was favoured with, the twilight will be seen extending over the dark hemisphere of the planet. This, and no more, is what I meant to express in one of my letters in which I mentioned the phænomenon in general, without any accurate computation, or particular induction.

But this pale luminous arc, *b c*, fig. 3, as it appears on the planet, not when it is at its greatest elongation, when we see half its illuminated surface, but at a time when it is approaching very near to its inferior conjunction, is in fact only the apparent, but by no means the real extent of the twilight, in a perpendicular direction east and west from the circle terminating the dark and illuminated hemispheres.

This real distance must evidently fall much short of the length of the abovementioned arc. The following is the method I have adopted to deduce the real extent of the twilight of Venus, from the apparent one as seen at the points of the cusps.

Let S, fig. 6, represent the sun, A D C B F the orbit of Venus, and T the place of the earth relatively to every aspect of Venus in its orbit. B will then be the point of inferior conjunction, C of the greatest western elongation, D of the superior conjunction, A of the greatest eastern elongation, and F nearly the point, not far distant from the inferior conjunction, at which I observed the planet on the 12th of March, 1792, when I measured the extent of its twilight.

Let also A B G C fig. 7, be the hemisphere of the planet, turned towards our earth T, and A F D E C the other hemisphere turned towards the sun S, at the time of an inferior conjunction. And let A C represent the terminating surface perpendicular to the plane A F D E C G B extending from the centre of the sun through that of the planet Venus, and the eye of the observer on our earth. A *f* *c* C will then be the mutual edge of the two hemispheres, and the whole of the dark one A B G C will be directed towards the eye of the observer at T.

But whenever Venus, before or after an inferior conjunction, comes into such a position, relatively with the sun and our earth, as to be illuminated by the former in the direction *b* E *c*, fig. 7, which position, on the 20th of March, 1790, must have been nearly at F fig. 6; then will F *g* *c* G, fig. 7, be the mutual terminating surface of the dark and light hemispheres of the luminous circumference; of which surface, however, only one half, *c* G, can be seen from the earth; the other half, F *g* *c*, lying in the hemisphere that is turned away from the eye: *c* will be the point of the cusp seen from T.

This being premised, let the twilight of Venus be so considerable as to extend, in a gradually-fading progression, from the luminous terminating border F *g* *c* G, to the line *h* *f* *k* *i*, which let us assume as the real border of the twilight. Then will a part of it, K *f*, terminating at *f* in the direction *a* *g*, parallel to *b* E *c*, the central ray from the sun, be seen projecting into the dark hemisphere of the planet, and this prolongation, as it was seen on the 12th of March, 1790, will be the apparent extent of the twilight; but its true extent will only be the distance *f g* measured in degrees of a great circle. And it is sufficiently evident, that the smaller we assume the angle D *c* E, before or after an inferior conjunction, the more pointed of course will be its equal angle A *c* F, and hence also the longer the visible part of the twilight *f c*. It is likewise manifest, that when the elongation of the planet is equal to the angle D *c* C, the line D *c k* B will represent the terminating surface of the dark and light hemispheres, whence the planet will, at its greatest elongation, be illuminated by the sun in the direction C *c*, and the true extent of the twilight will coincide with the apparent one on the terminating plane A *c*.

In order then to calculate in any, except the greatest, elongation, the arc *f g*, in the right-angled spherical triangle *f c g*, or to ascertain the true extent of the twilight in degrees of a great circle, it will be requisite to measure from T, the apparent prolongation of the twilight into the dark hemisphere *f c*, and the apparent diameter of the planet. With these *data* it follows that, as the angle *f g c* is a right angle, the angle *f c g* = A *c* F = D *c* E may be found by calculation, and the arc *f g* be thence easily deduced.

But the angle *f c g* = D *c* E is equal to the complement of the angle S F T, fig. 6, on the planet, to 180°, or F S T + S T F, as is plainly illustrated by fig. 10, Tab. VII. Let S be the sun, T our earth, *c* the centre of Venus; draw *g c* G perpendicular to S *c* K, and A *c* C perpendicular to T *c*: then is A *c* the projection of the visible terminating plane, as seen from T, and G *g* the projection of the luminous border. S *c* T is the angle on the planet, K *c* B its complement to 180°, and K *c* B = C *c* G;

$$\text{For, } E\,c\,G = A\,c\,B = 90^\circ$$
$$E\,c\,C = A\,c\,K$$
$$\text{And hence } C\,c\,G = K\,c\,B.$$

To find, therefore, the angle S *c* T, calculate for the 12th of March, 1790, at six o'clock in the evening.

The true heliocentric long. of Venus	$5^s\ 18^\circ\ 37'\ 35''$
Long. of the earth	5 22 21 20
Difference	3 43 45
Heliocentric lat. of Venus	3 23 0

$$\text{Log. sin. } 3^\circ\ 43'\ 45'' = \text{L. } 9{,}9990795$$
$$+\ \text{Log. cos. } 3\ 23\ 0 = \text{L. } 9{,}9992424$$
$$\text{Total} = \text{cos. } c\,S\,T = 9{,}9983219$$
$$= \text{L. } 5^\circ\ 2'\ 0''$$

Now in order to find the angle S *c* T, according to this heliocentric distance of Venus from the earth, we have in the triangle S *c* T, the angle at S, and the two sides *c* S and T S, being the distances of Venus and our earth from the sun. We then find the log. *c* S = 4857022, and the log. T S = 4997758, and the angle at S being = 5° 2′; the sum of the two angles will be = 174° 58′, the half of their sum = 87° 29′, and then the computation will be as follows:

Log. T S = 4997758
Log. c S = 4857022

Log. tang. 10,140736
belongs to 54° 7′ 30″
Subtract 45

9° 7′ 30″
Log. tang. 9° 7′ 30″ = 9205803
Log. tang. 87° 29′ = 11357018

Log. tang. semid. 10,562821
Half the difference of the angles = 74° 41′ 15″
Half the sum = 87 29 0

Angle S c T = 162° 10′ 15″
Angle c T S = 12 47 45

Now the angle at Venus being = 162° 10′ 15″. Its complement will be = 17° 49′ 45″. But this is = the angle C c G = fcg, fig. 7, Tab. VI. and hence we farther derive the sine of the arc $fg = \sin. fc \times \sin. fcg$; whence

Log. sin. 15° 19′ 0″ = 9,4218566
+ Log. sin. 17 49 45 = 9,4859768

Log. sin. fg = 8,9078334
which gives fg = 4° 38′ 30″.

Such, therefore, is the length of the arc of a great circle of Venus over which its twilight extends, as deduced from its appearance to the inhabitants of our globe under the most favourable circumstances: and if the true semi-diameter of the planet be assumed = 834 geographical miles, then will the extent of the twilight, in the perpendicular direction from the terminating border, amount to 67 of those miles: and this extent of twilight on Venus, which appears to us to measure 4° 38′$\frac{1}{2}$, may, with great propriety, be compared with what LAMBERT calls the common twilight on our globe, when its border passes immediately through the zenith, at a time when he estimates the place of the sun at 6° 23′$\frac{1}{2}$ below our horizon.*

This computation affords also the following conclusion: according to the measurement of the 12th of March, 1790, the projection of the twilight, or *f c*, fig. 7, amounted in its whole length to 8″; whence sin. *f c* × sin. 17° 49′ 45″, gives for the true extent of the twilight *f c* only 2,45 seconds of a degree; and hence it appears,

1. Why this twilight appears only at the points of the cusps and not at other parts of the terminating border, its extent, with its proportionably faint light, being too small; and this narrow faint light being, moreover, eclipsed by the superior brightness of the luminous hemisphere.

2. Why this twilight is distinctly seen only a few days before and after the inferior conjunction; the visible light of the luminous hemisphere being then only 2″ of a degree in breadth, and only the faint, declining, less luminous part of it apparent to the eye; whereas the projection of the twilight *f c*, fig. 7, will in this position be most extensive, and hence the more perceptible.

3. But should Venus at this time not be in the sign Aries, nor at so high a declination as when I saw it in March, 1790,

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* LAMBERT's Photom. §. 998, *sqq.* Berl. Ephem. for 1776.

so that the sun do not rise and set much earlier and later than the planet; and should, at the same time, our own crepuscular light be very great; should the planet, I say, when the sun is some degrees below our horizon, have no great altitude, it will then be either not seen at all, or only appear in our strong twilight at or near the horizon. As, even besides all these favourable circumstances, it is moreover requisite that the atmosphere be uncommonly clear, it cannot be thought extraordinary that this very instructive phænomenon, on which I never reflect without particular satisfaction, should not have been seen by me sooner in the long and uninterrupted series of observations I have made, but it will rather be allowed to have been a most fortunate combination of incidents which enabled me to observe it for four days successively. It will also appear,

4. Why this phænomenon did not shew itself again after the conjunction with the same degree of splendour it did before; the northern declination of the planet being then daily decreasing, and the length and brightness of our twilight as gradually increasing. This will also point out the reason why it may not be seen again for some time. The fact, however, being now known to exist, there is no doubt but that some traces of it may, and I flatter myself will, be perceived, even under other perhaps less favourable circumstances.

This discovery, lastly, throws also some light on the constitution of the atmosphere of Venus, and the physical construction of that planet.

It appears, from the abovementioned observations, that however faint the last perceptible degree of evanescent light be in this phænomenon, we do not however, even with the keenest eyesight, aided by the very best telescopes, distinguish the final termination of the twilight, but can only ascertain the border where its most luminous part begins to fade. If it be then mathematically demonstrated that this more luminous part, which may in some respects be compared to our own twilight, does actually, a few possible but trivial errors in the measurement excepted, extend 4° 38′ 30″ into the dark hemisphere, there can be no doubt but that this twilight does, in its whole extent, approach nearly to the extent of ours, which has been estimated at 18°; and that there is hence a material similarity between the two.

This may be inferred from the uncommon brightness of the solar light on this planet, and the comparatively very bright appearance of the more luminous measured part of its twilight, which also accounts for our seeing this more luminous part so distinctly even in the midst of our own twilight, and perhaps points out why even the pale bluish-grey light which in this, as well as in other proper phases, is seen to terminate the luminous part, forming an evanescent margin of 1 or 1½ seconds, may afford some indication of a twilight gradually blended with the luminous terminating border. When this twilight was measured on the 12th of March, 1790, soon after six P. M. about twenty-six minutes after sun-set, the sun was at most only 4° below our horizon, and the angle *c* T S, fig. 10. Tab. VII. could, according to the above calculations, not measure above 12° 47′ 45″.

Hence it appears that, under such circumstances, the evanescent margin at the termination of the brighter visible part of the twilight of Venus, was at least as luminous as our own crepuscular atmosphere at that point of the heavens. And it will also be easily perceived, with respect to the much stronger solar light on Venus, that if those last layers of its atmosphere, on which even the least vestige of the solar light can be traced, be transferred in idea into the atmosphere of our earth, admitting a similar density, but a less power of solar light, they would certainly exhibit no such perceptible traces.

And if it be probable, to a degree of evidence, that the twilight on Venus is nearly equal in its extent to that on our globe; if it be mathematically proved that its brighter part, which has so much light as to enable us not only to see it, but even to measure it across our fainter twilight, illuminates a zone 4° 38′ 30″ in breadth, we may hence surely infer the density and perpendicular height of at least the inferior more condensed part of the atmosphere, which is capable of reflecting such a crepuscular light; and apply those principles according to which the height of our own atmosphere, in as far, namely, as its lower strata are able to reflect the solar rays, and occasion a twilight, has been estimated at 34585 toises, or nearly nine geographical miles.

Let *a k b e f l*, fig. 9, Tab. VII. be the surface of Venus; *k b e*, an arc of it, which, according to the above calculation of the extent of the brightest twilight on the planet, is assumed at 4° 38′ 30″; *b d*, the height of that denser part of the atmosphere which actually occasions the twilight that is visible to us; and *b d*, the first ray of the sun, at this time 4° 38′ 30″ below the horizon, which is reflected by the highest stratum of the denser atmosphere *d*, upon the point *e* on the surface of the planet. We then have, in the triangle *d e c*, the side *c e*, or the semidiameter of Venus $=$ 834 geogr. miles, the right angle *e*, and the angle $c = 2° \ 19' \ 15''$, or half the angle of the twilight. And the side *d c* being to *e c* as rad. is to the cosin. of the angle *c*, subtract from this *b c*, the semidiameter of Venus, and the remainder, *d b*, will be the height of the atmosphere. Now, taking 834 geogr. miles = 3178874 toises for the semidiameter, we have

$$\begin{array}{lrl} \text{Log.} & 3178874 = & \text{L. } 6{,}5022632 \\ \text{Log. cos. } 2^\circ\ 19'\ 15'' & = & \text{L. } 9{,}9996436 \\ \hline \text{Log. } d\,c & = & \text{L. } 6{,}5026196 = 3181400 \\ & & \phantom{\text{L. } 6{,}5026196} - b\,c = 3178874 \\ \hline & & \phantom{\text{L. } 6{,}5026196 -}\ d\,b = \quad 2526 \end{array}$$

Hence it appears that the perpendicular height of the inferior and more dense part of the atmosphere of Venus, which has the power of reflecting the solar light to such a degree, as, under favourable circumstances, to be visible on our globe, where, with a good telescope, it assumes the appearance of a faint ash-coloured light, measures 2526 toises, or 15156 Paris feet.

As we are not acquainted with the laws of the inflection of light at the atmosphere of Venus, and are as much in the dark, concerning that as well as our own twilight, whether it be produced by a simple or compound reflection, this computation, it must be owned, is somewhat conjectural, and at best, conditional; and yet it supplies us with some farther means of estimating the height and density of the atmosphere of Venus with a somewhat greater precision.

If, according to the above principles and computations, it be determined that the twilight of Venus extends, at least in a considerable proportion, as far as ours; and if the inferior part of the atmosphere, as far as a perpendicular height of 15000 feet, be so dense as to reflect so clear a light upon a zone extending 67 geogr. miles into the dark hemisphere, that we, at a distance of only 12° 47′ 45″ from the sun, and when this luminary is only from 3 to 4 degrees below the horizon, can see it so distinctly and evidently as to be able to compare it to our common twilight, measuring 6° 23′$\frac{1}{2}$, it must follow, that the atmosphere of Venus rises like ours, far above the highest mountains: and although we ascribe to that atmosphere the greatest possible transparency, it will still remain a more opake covering than, according to my Selenotopographical Observations, that of the moon appears to be. These observations also point out the probable cause why we do not see on this, our nearest, primary planet, even in the most favourable circumstances, any of the superficial shades, which, according to all analogy, we have reason to suppose on this, as well as on all the other planetary bodies; and which, considering the proximity of the planet, we should, when at its mean distance, and with a good HERSCHELLEAN telescope, magnifying 60 or 70 times, certainly distinguish nearly as plainly as I have seen, even with the naked eye, the superficial shades on the moon, not only the greatest ones, but also some of the lesser, such as Copernicus, Aristarchus, &c.; and surely, as well as we can discern, with a proper luminous magnifying power, the variable faint atmospheric stripes on Saturn and Jupiter, and the atmospheric opacities on Mars. At least it appears to me, that the density of the lower part of Venus's atmosphere, which enables it to reflect so clear a light into the dark hemisphere, sufficiently warrants such a conjecture: and it may well be conceived, that with so dense an atmosphere, the very strong light of Venus must occasion a much greater degree of glare, than the soft light of the moon in a much more rarified atmosphere. In general it appears to me, that whoever will bestow some attention on these observations, and compare them impartially with the many others I have made on the atmospheres of the moon, Jupiter, Saturn, and even the sun, will find a striking coincidence between them.

The simple fact, that Venus in its different phases, and especially at the times of its greatest elongations, shews a much greater diminution of light at its terminating border than the moon, is, no doubt, sufficient to point out a much denser atmosphere than that of this satellite. And this is, moreover, fully evinced by the observations on its twilight. The circumstance also, that there are seen on this planet none of the flat spherical forms as are conspicuous on Jupiter and Saturn, none of the stripes or longitudinal spots parallel to the equator, which are seen on these planets and the sun, and which point out a certain stretch of atmosphere, gives room to infer, that the globe of Venus, with reference to its diameter, and other circumstances of physical arrangement in its construction,* performs its rotation round its axis in a much longer space of time than those planets, or the nearly similar ones of our earth and Mars; and this is actually confirmed by my observations on the diurnal period of Venus.

Thus we see a remarkable coincidence in every respect; and yet, though we cannot suppose a smaller, but rather a greater force of gravity on the surface of Venus than on our globe, nature seems, however, to have raised on the former such great inequalities, and mountains of such enormous height, as to exceed 4,5, and even 6 times the perpendicular elevation of Cimboraco, the highest of our mountains. Thus are we, by these observations, led to a farther contemplation of the immense, and yet analogical variety with which the great Author of nature has dignified his works, as well in the greater objects, as in the smallest microscopic atoms; and the incessant novelty of combinations with which he has adorned them.

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* These references have already been noticed by Chevalier DE GOIMPY, in the *Journal des Savans* for January, 1769; and before him, by M. DE MAIRAN, in the *Memoires Acad.* for 1729. See DE LA LANDE'S Astron. §. 3219, and 3122.

On the Atmosphere of the Moon.

Referring to my Selenotopographic Fragments for the proofs I there adduced of the real existence of a lunar atmosphere, which had been so frequently doubted; I shall also appeal to the same work for the observations I formerly made on several of its relative properties, compared with the same in our atmosphere, such as its greater dryness, rarity, and clearness, which, however, do not prevent its refracting the solar rays, having pointed out the circumstance, that the mountains in the dark hemisphere of the moon, near its luminous border, which are of sufficient height to receive the light of the sun, are the more feebly illuminated the more distant they are from that border: from which proofs of a refracting atmosphere, I also deduced the probability of the existence of a faint twilight, which, however, my long series of observations had not yet fully evinced.

As one fortunate discovery often leads to another, I had no sooner succeeded in my observations on the twilight of Venus, than I directed my attention to that of the moon, and applied the calculations and inferences I there made, to some appearances I had already noticed on this satellite. It occurred to me, that if in fact there were a twilight on the moon, as there is on Venus and our earth, it could not, considering the greater rarity of its atmosphere, be so considerable: and that the vestiges of it, allowing for the brightness of the luminous part of the moon, the strong light that is thence thrown upon the field of the telescope, and in some measure the reflected light of our earth, could only be traced on the limb, particularly at the cusps; and even this only at the time when our own twilight is not strong, but the air very clear, and when the moon, in one of its least phases, is in a high altitude, either in the spring, following the sun two days after a new moon, or in the autumn, preceding the sun in the morning, with the same aspect: in a word, that the projection of this twilight will be the greater and more perceptible the more falcated the phase, and the higher the moon above the horizon, and out of our own twilight. This struck me the more, as I recollected having, two years ago, perceived such an appearance at the outward edge, near the points of the cusps, though I did not then reflect on the cause of it.

As all the requisite circumstances, however, do not often coincide, I thought myself particularly fortunate when, on the 24th of February, I was favoured with a lucky combination of them. Although this be as yet only a single observation of the sort, it is, however, in every respect so complete, and the inferences it leads to are, to me at least, so new and interesting, that I cannot withhold it from those liberal minded men, who are zealous in the pursuit of genuine philosophical knowledge.

On the abovementioned evening, at 5^h 40′, two days and twelve hours after the new moon, when in consequence of the libration, the western border of the grey surface of the Mare Crisium was 1′ 20″ distant from the western limb of the moon, the air being perfectly clear, I prepared my seven-feet reflector, magnifying seventy-four times, in order to observe the first clearing-up of the dark hemisphere, which was illuminated only by the light of our earth, and more especially to ascertain whether in fact this hemisphere, which, as is well known, is always somewhat more luminous at the limb than in the middle, would emerge out of our twilight at many parts at once, or first only at the two cusps. Both these points appeared now, most distinctly and decidedly, tapering in a very sharp, faint, scarce any where interrupted, prolongation; each of them exhibiting, with the greatest precision, its farthest extremity faintly illuminated by the solar rays, before any part of the dark hemisphere could be distinguished. But this dark hemisphere began soon after to clear up at once at its border, though immediately only at the cusps, where, but more particularly at their points, this border displayed, on both at the same time, a luminous margin, above a minute in breadth, of a very pale grey light, which, compared with that of the farthest extremities of the cusps themselves, was of a very different colour, and relatively as faint as the twilight I discovered on the dark hemisphere of Venus, and that of our own earth, when compared with the light immediately derived from the sun. This light, however, faded away so gradually towards the east, as to render the border on that side perfectly undefined, the termination losing itself imperceptibly in the colour of the sky.

I examined this light with all possible care, and found it of the same extent at both points, and fading away at both in the same gradual proportion. But I also, with the same caution, explored whether I could distinguish any part of the limb of the moon farther towards the east; since if this crepuscular light had been the effect of the light reflected from our globe, it would undoubtedly have appeared more sensibly at the parts most remote from the glare of the illuminated hemisphere. But, with the greatest exertion of my visual powers, I could not discover any part of the, as yet, wholly darkened hemisphere, except one single speck, being the summit of the mountainous ridge Leibnitz, which was then strongly illuminated by the solar light: and indeed eight minutes elapsed before the remainder of the limb became visible; when not only separate parts of it, but the whole displayed itself at once.

This alone gave me certain hopes of an ample recompence, and satisfied me that the principles I had laid down in my Selenotop. Fragm. § *525, seqq.* concerning the atmospheres of the planets, and especially of the moon, are founded on truth. But a similar observation made on the 6th, after seven o'clock, afforded me several collateral circumstances, which strongly corroborate what I have there advanced on this subject. The whole limb of the dark hemisphere illuminated only by the reflected light of our globe, appeared now so clear and distinct, that I could very readily discern not only the large, but also the smaller spots, and among these Plato, Aristarchus, Menelaus, Manilius, Copernicus, &c. and even the small speck to the north-west of Aristarchus, marked *b*, Tab. XXVII. fig. 1. of the Fragments. I could apply the usual power magnifying 161 times; and had full leisure, and the means to examine every thing carefully and repeatedly, and to take very accurate measurements.

Although a just idea of so delicate a phænomenon as this crepuscular light cannot possibly be conveyed by a drawing, but must be gathered from actual inspection, I have nevertheless attempted a delineation of it, and of the southern and eastern cusps fig. 1 and 2, Tab. VIII. as deduced from my measurements, especially at the southern cusp, in hopes thereby to render what I have farther to say concerning this observation the more intelligible.

The southern cusp, fig. 1. extended from *a* to *c*, with a gradually fading but still resplendent solar light, of its usual pale yellow colour, and terminated at *c* with a mountain. That this was really the point of the cusp, appears not only from the general construction of the falcated segment, which was sufficiently narrow even at its beginning *a*, near which it was somewhat disfigured at *b* by a high mountain; but also from the narrowness of its luminous curve at *d e* and *f*, the breadth of which seldom exceeded 1″, and had a sensible interruption so near as *d*. This curve was throughout, from *a* to *c*, except where the glare of the solar rays spread some degree of light, bordered with the pale ash-colour of the dark hemisphere, glimmering with the faint light reflected from our earth, out of which, however, rose the higher mountains *g*, *h*, *i*, *c*, which were now already illuminated by the sun; and farther on, not less than thirty lines, or, according to my usual projection, two minutes, distant from the point *c*, was seen another mountain *l*, which belonged to the high ridge Leibnitz, and also received its light immediately from the sun.

There can hence be no doubt of the termination of the cusp being at *c*: and this being well ascertained, I now distinguished with the greatest certainty the twilight extending from *c* to *k*. The most remarkable circumstances attending this light were, that it was broadest and brightest at *c*, and that it dwindled away and contracted towards *k*, where it lost itself in the faint glimmering of our terrestrial light; and that at the northern cusp, fig. 2, at which there do not appear to be so many mountains and inequalities as at the southern, this light exhibited the same pyramidal form, and was of equal length, and alike fading in intensity and colour, as that at the southern.

This light, compared with that of the thinnest and least bright part of the cusp *d c*, was as faint as the pale ash-coloured spots in the luminous hemisphere, when opposed to the bright ones. But this is still better illustrated by a comparison between the high mountain *l*, fig. 1, which now already appeared illuminated by the solar light, and the spot Aristarchus, which shone moderately merely with the light reflected from our globe. The said mountain had, comparatively with the thin luminous arc *d e f* of the bright hemisphere, and the mountains *g*, *h*, *i*, *c*, a very pale, fading, but yet brighter light than Aristarchus, as indeed might have been expected from what I said in my Selenotop. Fragm.; but this reflected light upon Aristarchus was however sensibly brighter than the glimmering light from *c* to *k*. And respecting the still fainter terrestrial light which bordered the luminous curve from *c* to *k*, fig. 1 and 2, I cannot give a better idea of it than by observing, that the light at the extremities of both the cusps appeared of a pyramidal form, similar to, but though gradually fading, and very undefined, yet brighter than that of our zodiacal light, when, in the months of March and April, it blends itself, comparatively with the remaining colour of the sky, with the terrestrial light, terminating in a very sharp point.

The undefined and gradually fading appearance of this light was the cause that, though I had recourse to a dark projection table, I could not, however, take any accurate measurements of it. I found, nevertheless, by repeated comparisons, that the length of this pyramidal glimmering light, in which I could perceive no sensible inequality at the limb of the moon, amounted to about $\frac{2}{3}$ of the distance between the two mountains *c*, *l*, fig. 1, which shone with the solar light. Comparing also this southern twilight with the northern, it appeared of the same length; and on measuring the distance *c l*, I found it repeatedly $=$ 30 lines $=$ 2′; so that the length of the twilight must have amounted to 20 lines $=$ 1′ 20″.

Its greatest breadth at *c* could, on the other hand, because of the extent and greater density of its light, be easily ascertained by means of the immediate application of the projection table. This measurement gave at most $\frac{2}{3}$ of a line, or full 2″.

Although I be positively certain of this very remarkable appearance at both cusps, and of its perfect similarity, in all my observations, I could not, however, trace any vestige of a like crepuscular light at any other part of the terminating border: nor could I on the very next evening, being the 25th, and also on the 26th of February, perceive, even at the cusps, any of the twilight I expected to see there; the very thin, faint, luminous line which did indeed appear on the 26th, at the southern cusp between *a* and *b*, fig. 3, being undoubtedly the effect of the immediate solar light, probably illuminating some prominent, flat area, as yet situated in the dark hemisphere.

Thus far the observations: and now for the application of them.

I need hardly insist upon the proofs, that the very faint pyramidal glimmering light, observed on the 24th of February, at the extremities of both cusps, could by no means be the immediate effect of the solar light, all the circumstances of the observations militating uniformly and decidedly against this supposition, which, were it true, would oblige us to admit a most unaccountable diminution of light, and thence also a density of the lunar atmosphere, that ought to exceed even the density of ours; a fact absolutely contradicted by all the lunar observations hitherto made. This light, indeed, was so very faint, that it disappeared at 7^h 20′, when the moon approached the horizon; whilst, on the other hand, Aristarchus, which had no light but what it received from the earth, was still very distinguishable; and the summit of Leibnitz *l*, fig. 1, (which, though far within the dark hemisphere, was, however, illuminated by the immediate solar rays) displayed a degree of brightness which, although when compared with that of the cusp *d e f*, it appeared very faint and dwindling, equalled, however, that of our Pic of Teneriffe. Nor can it be conceived why this glimmering light broke off so suddenly at both the cusps, without a progressive diminution. It can hardly be supposed that similar, grey, prominent, flat areas, of the same form and dimensions, and comparatively of a faint light, which, whilst in the dark hemisphere, they derive immediately from the sun, exist on all parts of the moon; more especially as at the places observed, the limb happened to exhibit throughout an exact spherical form, without the least sensible inequality; and as in both the bordering regions of the northern and southern hemispheres, especially in the latter, no such grey, prominent planes are any where discernible. It may then be asked, why did this faint glimmering light appear at both cusps, along equal arcs of the limb, of equal length and breadth, and of the same pyramidal form? and why did its farther extremity blend itself with the terrestrial light of the dark hemisphere, which, according to a great number of my selenotopographic observations, is by no means the case, even with those grey prominent areas which, being at some distance on the dark side of the terminating border, are nevertheless illuminated immediately by the sun?

These, therefore, could certainly not derive their light immediately from the sun; whence this appearance, like the similar ones on the planet Venus, can only be ascribed to the solar rays reflected by the atmosphere of the moon upon those planes, producing on them a very faint, gradually diminishing, glimmering light, which at last loses itself in the reflected terrestrial light, in the same manner as our twilight blends itself with the light of the moon. Every circumstance of the above observation seems to me to confirm this supposition; and hence the observation itself, which, though single, was however a most fortunate and complete one, must appear of no small degree of importance, since it not only confirms the observations and inferences on the long contested lunar atmosphere contained in my Selenotop. Fragm. but also furnishes us with many more lights concerning the atmosphere of planets in general, than had been afforded us by all those observations collectively.

This, and the mathematical certainty that the phænomenon is, in fact, nothing but a real twilight in the lunar atmosphere, will be farther evinced by the following theoretical deductions, in which the same mode of computing as was used in the observations on Venus, will be adopted, with this difference, that the angle *f c g*, fig. 7, Tab. VI. = A *c* F, is equal to the angle D *c* E, or the geocentric elongation of the moon from the sun.

1. According to the above observation, we find that the twilight at both cusps measured, from *c* to *k*, fig. 1 and 2, Tab. VIII. 20 lines = 1′ 20″. The apparent diam. of the moon being at 6^h P. M. = 31′ 18″; whence the twilight must have extended over an arc of the limb, measuring 4° 53′ 23″; and, if the real diam. of the moon be assumed at 465 geogr. miles, this extent must have answered to $19\frac{3}{4}$ of these miles.

2. But we have seen that *f c*, fig. 7, Tab. VI. or the measured projection of the twilight, is by no means the true distance of its farther edge *f*, from the terminating border *g*. This distance, therefore, must be found by the following computation:

Long. of the moon, Feb. 24, 6^h P. M. = 12^s 7° 39′ 22″
Long. of the sun - - - - = 11 5 52 22

Difference of Long. = 1 1 47 0
= 31° 47′ 0″
Latitude of the moon = 0 22 17

Log. cos. 31° 47′ 0″ = L. 9,9294424
+ Log. cos. 0 22 17 = L. 9,9999908

Log. dist. of ☽ from the ☉ = L. 9,9294332
= 31° 47′ 7″

Thus was the above the amount of the angle fcg, fig. 7; and as the sin. of the arc $fg = \sin. fc \times \sin. fcg$, then

$$\begin{array}{lllll} & \text{Log. sin.} fc & = \text{L. } 4^{\circ}\ 53'\ 23'' & = \text{L. } 8{,}9306436 \\ + & \text{Log. sin.} fcg & = \text{L. } 31\ \ 47\ \ 7 & = \text{L. } 9{,}7215942 \\ \hline & & \text{L. sin.} fg & = \quad 8{,}6522378 \\ & & \text{which gives } fg & = 2^{\circ}\ 34'\ 25'' \end{array}$$

Whence I infer, that the breadth of the lunar twilight from the terminating border to where it loses itself in, and assumes the faint appearance of, the light reflected from our earth, measures, in a direction perpendicular to the aforesaid border, 2° 34′ 25″, equal to a breadth of 105 lines, or $10\frac{1}{3}$ geogr. miles.

3. Hence, admitting that the laws of the inflection of light are the same in the moon as on our earth and Venus, it will be easy to compute the perpendicular height of the inferior and more dense part of the lunar atmosphere $b\,d$, fig. 9, Tab. VII.; the angle c, in the right angled triangle $d\,c\,e$, $= 1^{\circ}\ 17'\ 6''$, and the radius $b\,c = 234$ geogr. miles, or 891914 toises, being given; whence

$$\begin{array}{llll} & \text{Log. rad. } \rightmoon = \text{L. } 891914 & = \text{L. } 5{,}9503210 \\ - & \text{Log. cos. } 1^{\circ}\ 17'\ 6'' & = \text{L. } 9{,}9998908 \\ \hline & & 5{,}9504302 & = 892140 \\ & & -\,b\,c & = 891914 \\ \hline & & d\,b = & \quad 226 \end{array}$$

So that the inferior more dense part of the moon's atmosphere, measures, in perpendicular height, not more than 226 toises, or 1356 Paris feet; that inferior part, namely, which reflects so much of the solar light into its dark hemisphere as produces a twilight, more luminous than the light reflected upon its surface from the earth, when it is only 31° 47′ 9″ from its opposition to the moon, when, consequently, it shews the greatest part of its disk illuminated, and affords some degree of light to the nights in the moon.

A cursory survey of these computations, shews that they not only contribute to illustrate the observations, but also that they coincide so fully with the collateral circumstances, as still farther to confirm those observations.

1. I was well aware, even at the time I made the observations, that the lunar twilight must, as it actually did, appear in the form of a lengthened pyramid. And indeed the 7th fig. Tab. VI. which represents the twilight *c k*, *g f*, F *b*, before the conjunction, projecting from the cusps towards the west, clearly points out, that only its pyramidal part *c f k* could be visible at T, whilst the whole remainder of it F *g c f b* extended over the whole hemisphere of the moon A F D E C, which was turned away from the eye of the observer.

2. It appeared to me, on the other hand, before I had sufficiently reflected on these collateral circumstances, that it could not well be reconciled to theory, that this twilight, whose length from *c* to *f* measured 20 lines, should at *c k*, where its breadth was the greatest, not be broader than $\frac{1}{2}$, or at most $\frac{2}{3}$ of a line. I soon, however, removed the difficulty by the following deduction. From the above computation, it appears that the side $f g = k c$, or the distance of *f k*, the farther edge of the twilight from the terminator *g c*, was = 105 lines, or an arc of 2° 34′ 12″. But the arc $k c = f g$, or the greatest visible

breadth of the twilight did not lay, like the side fc, along the edge of the limb, but nearly at right angles to it, in a direction immediately tending towards the eye: whence its orthographic projection could by no means amount to 10,5 lines, but must, according to well known principles, be less in the proportion of the sine of 2° 34′ 12″ to the cosine = 10,5, whence, by the following computation,

Log. $k\,c = f\,g$ = L. 10,5 = L. 2,0211893
× Log. sin. 2° 34′ 12″ - - = L. 8,6516648
——————— = L. 0,5

This greatest apparent breadth of the twilight at the cusps could not, therefore, exceed 0,5 lines, or full two seconds; and this in fact was the breadth given it by the observation. It is scarce possible, that in the determination of so nice an object, theory should coincide more accurately with observation.

3. But it appears farther from these observations, why, notwithstanding all the pains I took, I could perceive nothing of this twilight at the terminating border of the falcated phase, and why, on the succeeding evening, this light disappeared likewise at the cusps. The brightness of the phase will, even on the first days after the new moon, occasion a strong glare in the field of the telescope, and this particularly at the spherical border, terminating the light; so that the margin contiguous to this border, which is only illuminated by the light reflected from the earth, may, indeed, appear luminous, but must necessarily be very undefined. The smaller the phase, moreover, the less will be the projection of the twilight at the terminating border, so that on the third day after the new moon it cannot well amount to much more than 2″ at the cusps; and it will then be so far obscured by the stronger

light of the illuminated hemisphere as to be almost evanescent. The best illustration of this, and of the known fact, that in the broader phases the twilight will, on account of the strong contrasting light of the luminous part, vanish even at the cusps, is given by the spots Manilius and Menelaus. Both are situated near the centre of the hemisphere turned towards our earth ; both have considerable diameters, the one of 24 and the other of between 14 and 16 seconds, and both appear very distinctly on the third day after the new moon, with a degree of light rather surpassing that of the twilight. These spots, however, notwithstanding their great diameters, vanish on account of the increasing glare of light, so soon as four days after the new moon, although they be then still at some distance from the terminating border.* How could, therefore, a faint light, measuring only 2″ in breadth, be visible on the third day after a new moon, at the terminating border? And how could it at the fourth day still appear distinctly at the bright cusps, where it is, indeed, of a more considerable extent, but still must, in consequence of its pyramidal form, soon dwindle away into a very narrow tapering stripe? This, moreover, was precisely the case in the many observations I made on the planet Venus.

From the above calculations it also appears, that the lower and more dense part of the lunar atmosphere, that part, namely, which has the power of reflecting this bright crepuscular light, is only 1356 Paris feet in height ; and hence it will easily be explained how, according to the different librations of the moon, ridges of mountains even of a moderate height, situated at or near the terminating border, may partially interrupt, or, at times, wholly prevent this crepuscular light, either at one or the other cusp, and sometimes at both. I cannot hence but consider the discovery I here announce as a very fortunate one, both as it appears to me decisive, and as it may induce future observers to direct their attention to this phænomenon.

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* See Selenot. Fragm. §. 531.

Admitting the validity of this new observation, which, I think, cannot well be called in question, I proceed now to deduce from it the following inferences.

1. It confirms, to a degree of evidence, all the selenotopographic observations I have been so successful as to make, on the various and alternate changes of particular parts of the lunar atmosphere. If the inferior and more dense part of this atmosphere be, in fact, of sufficient density to reflect a twilight over a zone of the dark hemisphere 2° 34′, or 10¾ geogr. miles in breadth, which shall in intensity exceed the light reflected upon its dark hemisphere by the almost wholly illuminated disk of our earth; and if, by an incidental computation, this dense part be found to measure 1356 feet in perpendicular height, it may, according to the strictest analogy, be asserted, that the upper, and gradually more rarified strata, must, at least, reach above the highest mountains in the moon. And this will appear the more evident, if we reflect that, notwithstanding the inferior degree of gravitation on the surface of the moon, which NEWTON has estimated at somewhat less than $\frac{1}{6}$ of that on our earth, the lower part of its atmosphere is, nevertheless, of so considerable a density. This considerable density will, therefore, fully account for the diminution of light observed at the cusps, and on the high ridges Leibnitz and Doerfel, when illuminated in the dark hemisphere; as

also for the several obscurations and returning serenity, the eruptions, and other changes I have frequently observed in the lunar atmosphere. This observation also implies,

2. That the atmosphere of the moon is, notwithstanding this considerable density, much rarer than that of our earth. And this, indeed, is sufficiently confirmed by all our other lunar observations. I think I may assert, with the greatest confidence, that the clearer part of our twilight, when the sun is 4° below our horizon, and when we can conveniently read and write by the light we receive from it, surpasses considerably in intensity the light which the almost wholly illuminated disk of our earth reflects upon the dark hemisphere of the moon $2\frac{1}{2}$ days before and after the new moon. But should we even admit an equal degree of intensity, it will, however, appear from computation that our inferior atmosphere, which reflects as strong a light over 4° as that of the moon does over 2° 34′ of their respective circumferences, must be at least eight times higher than that of the moon.

3. The striking diminution of light I noticed, in my twelve years observations on Venus, likewise indicates that the atmosphere of that planet, which is in many respects similar to ours, is much denser than that of the moon; and this will be still farther corroborated, if we compare together the several measurements and computations made concerning the twilights of different planets. There is no doubt but that the faintest twilight of Venus, as seen either before or after the rising and setting of the sun across our twilight, is much brighter than that of the moon; and it appears, moreover, from computation, that the denser part of the atmosphere of Venus measures at least 15000 Paris feet in height, and spreads

its twilight 67 geogr. miles into the dark hemisphere, whilst the denser part of the lunar atmosphere, whose height does not exceed 1356 feet, produces a faint twilight not above $10\frac{1}{3}$ geogr. miles in breadth. Thus, as my successful observations on the twilight of Venus led me to the discovery of that of the moon, so did these latter reciprocally confirm the former: and thus, which ever way we contemplate the subject, must we be struck with the coincidence that prevails throughout.

4. But if the lunar atmosphere be comparatively so rare, it follows that the inflection of light produced by it cannot be very considerable; and hence does the computation of M. DU SEJOUR, according to which, the inflection of the solar rays which touch the moon, amounts to no more than $4''\frac{1}{2}$, receive an additional degree of authenticity.[*] Besides which,

5. As the true extent of the brightest lunar twilight amounts to $2^\circ\ 34'$, the obliquity of the ecliptic in the moon only to $1^\circ\ 29'$; the inclination of the orbit of the moon, on the contrary, to $5^\circ\ 15'$, and its synodic period, during which it performs a revolution round its axis is $= 29^d\ 12^h$; it follows, that its brightest twilight, to where it loses itself in the light reflected by the almost fully illuminated disk of our earth, must, at least at its nodes, last $5^h\ 3'$, and that it will be still longer at other parts of the orbit, according to the situation of the nodes.

6. And lastly, it being a well known fact[*] that the fixed stars, as they approach the moon, diminish in splendour at the most only a very few seconds before their occultations, it was

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[*] See DE LA LANDE's Astron. §. 1992—1994.

[†] See Selenot. Fragm. §. 531, with its note.

natural for me, after the successful observations I had made on the twilight of the moon, to pay particular attention to this circumstance. On the 25th February, at 6^h P. M. the sky being very clear, the limb of the dark part of the moon appeared uncommonly distinct; and only a few seconds of a degree from its edge was seen a telescopic star, of about the 10th or 12th magnitude. I counted full 20″ before its occultation, and $18\frac{1}{2}$ of these, without the least perceptible diminution of light. The star, however, began now gradually to fade, and after the remaining $1''\frac{1}{2}$, during which I observed it with all possible attention, it vanished in an instant. This observation agrees perfectly with the above computations. Although it be proved that the inferior dense part of the lunar atmosphere reflects a stronger light than that which the dark hemisphere receives from an almost fully illuminated disk of our earth; and although, considering the inferiority of gravitation on the surface of the moon, there be no doubt that this dense part, together with the superior gradually more rarified regions of its atmosphere, must extend far above its highest mountains; it is yet a fact that the breadth of this observed twilight, to where it loses itself in our reflected terrestrial light, does not measure more than 2° 34′: it is, therefore, highly probable that its greatest extent, in the most favourable phases near our new moon, can never exceed the double of the above arc, or 5° 8′; and hence we can only infer a perpendicular height of an atmosphere, capable of inflecting the solar rays, which at most measures 5376 feet: nor is it very likely that, unless accidental, and hitherto unknown circumstances should occasionally condense different parts of this atmosphere, these upper strata should materially affect the distinctness of a star seen through it.

But admitting the height of the atmosphere, which may affect the brightness of a fixed star, not to be less than 5376 feet, this will amount to an arc of only 0,94″, or not quite one second; and as the moon describes an arc of 1″ in 2″ of time, it follows that in general the fading of a star, which approaches to an occultation, cannot last quite 2″ in time; that if the appulse be at a part of a limb of the moon where a ridge of mountains interferes, the gradual obscuration will last a still shorter time; and that it may, under some circumstances of this nature, be even instantaneous.

Supplement to the foregoing Observations on the Atmosphere of the Moon.

The following observation of an occultation of Jupiter by the moon, when near its full, occurred to me by mere accident, on the 7th of April, 1792; and I was the more gratified by it, as I had long wished for an opportunity to observe such a phænomenon, and had no expectation of seeing it at this time, no mention being made of it in the Ephemerides.

The sky being very serene, and Jupiter uncommonly bright, I prepared my seven-feet reflector, magnifying 74 times, in hopes that the strong light and distinctness it afforded would enable me to compare the appearances of this phænomenon with the results which I had deduced from my late observations on the height and density of the atmosphere of the moon.

Fig 1. Tab. IX. represents the situation of Jupiter's four satellites, as they appeared, most distinctly, two of them to the westward, the second about one, and the first near two of Jupiter's diameters distant from its limb; and the two others

to the eastward, the third about seven, and the fourth near eight of the same diameters, distant from the said limb.

Fig. 2. shews Jupiter, with its belts, and of a somewhat spheroidical form, as it now appeared to me, and as distinctly as I had ever seen it. The equatorial belt, from *a* to *d*, was very apparent. It consisted properly of two zones, *a b*, and *c d*, of a brownish-grey colour, with a more luminous interval *b c*, between them. At *e* and *f* were two comparatively well defined stripes, which I had noticed for many years back, but which now crossed the whole disk; and the polar regions appeared again, from *g* and *h*, more dim and grey than the bright part of the planet. But what particularly struck me, were two nebulous undefined spots, *i* and *k*, which were sensibly darker than the principal zone *d d*; and at *l*, a still more remarkable, circular, though imperfectly defined spot, somewhat brighter than the luminous interval between the zones, and perfectly similar to the remarkable luminous spot which I had observed in 1786 and 1787, on the same part of Jupiter, and which then led me to some very unexpected inferences concerning the atmosphere of that planet.*

These favourable circumstances led me to the following accurate observation, which I was certain would prove instructive to me. At $10^h\ 40'\ 50''$, I saw the spot *i* at about the middle of its parallel; and immediately after began the occultation, than which a more distinct and beautiful one was, perhaps, never seen.

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* *Beytr. zu den neuesten Astron. Entd.* p. 75. and Tab. I. fig. 6.

Immersions.

The western, preceding, first satellite disappeared behind the sharp bright limb of the moon, at 10^h 43′ 12.*

N. B. There may here have been an error of two or three seconds, as the satellite became indistinct at the limb.

The second satellite disappeared, without becoming at all indistinct, exactly at 10^h 45′ 19″.

The western limb of Jupiter came in contact, most distinctly, with the eastern limb of the moon, at 10^h 46′ 32″,5.

Jupiter's eastern limb disappeared, as distinctly, at 10^h 48′ 20″,5.

N. B. This immersion took place, as represented in fig. 3, to the eastward of Aristarchus, at about the 24th degree of N. lat.

The third satellite disappeared, after having been for about one or two seconds faint and indistinct, at 10^h 58′ 57″,5.

The fourth satellite, which appeared the least of them all, became undiscernible near the limb, and vanished at about 11^h 2′ 16″.

N. B. There may have been an error of four or five seconds.

Emersions.

The two preceding first and second satellites were here likewise of use in determining precisely the emersion of both the limbs of Jupiter, from the dark hemisphere of the moon.

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* True time, determined by equal altitudes, taken on the 12th and 8th of April, and 2d and 27th of March, and by the known rate of the clocks; but as no altitudes could be taken on the 7th, there may be an error of two or three seconds.

The first appearance of Jupiter's western limb was very distinct at $11^h\ 43'\ 54''$.

Emersion of the eastern limb, as distinct, at $11^h\ 45'\ 39'',5$.

N. B. This emersion took place, as represented in fig. 4, to the north-eastward of Seneca (B, Tab. VIII. of the Frag.), at about the 23d degree of N. lat.

The emersion of the next, or third satellite, was not observed.

That of the fourth was distinct, at $11^h\ 59'\ 1''$.

This observation gave me the more satisfaction, as it singularly contributed to confirm the discovery I had been so fortunate as to make of the twilight in the moon, and the height and density of the lower stratum of its atmosphere.

Experience has sufficiently proved, that a stronger will ever obscure a fainter light; and it follows hence, that the light of a bright star approaching the moon, when full, or nearly so, will lose something of its lustre: but little can be inferred in favour of an atmosphere either of the moon, or of Mars, from the observation of CASSINI, in which, as Dr. HERSCHEL has illustrated by some observations of his own,* a star in Aquarius, of the sixth magnitude, and as yet six minutes distant from Mars, diminished in light when both were seen in the same field of the telescope. A mere apparent diminution of light, occasioned by the glare of a larger luminous object, when seen at the same time with a smaller one, in the field of the telescope, is one thing; and another thing is a real indistinctness of the small luminous body, which increases in proportion as they approach nearer to each other.

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* Phil. Trans. for the Year 1784, Part II. p. 271.

It was very natural for Jupiter to diminish in brightness, when it approached so near to the moon, then almost at its full, as to be seen at the same time in the field of the telescope, which was, in fact, the circumstance of this observation; but I could not observe any progressive variation of light in the eastern and western, equally luminous, disks, proportional to their distances from the limb of the moon, much less a real indistinctness; and this neither when the limbs of the two planets were nearly in contact, nor when Jupiter was partly, or about one half, covered by the moon

It was a sight truly gratifying to an eye accustomed to the light of the moon, or in general to similar observations, to behold how Jupiter, at its immersion, as well as emersion, being half, or more than half, covered by the moon, exhibited its belts and other parts, as distinctly, close to the limb of the moon, as it does at some distance from it: and had I not already succeeded in my numerous observations on the atmosphere of the moon, and very recently in those which enabled me to determine its twilight, I should perhaps have adopted the doubts the ancient astronomers entertained concerning the existence of a lunar atmosphere; and this the rather, as when Jupiter, in its immersion, was so far covered, that the luminous spot *l*, fig. 2, was close to the moon, I could plainly distinguish this spot, although it be in itself by no means very perceptible.

Such, however, must have been the appearances, according to my new observations and measurements of the twilight of the moon: for if it be proved that the extent of this twilight, to where it loses itself in the light reflected from the almost wholly illuminated disk of our earth, amounts to no more than an arc of 2° 34′ of the circumference of the moon, and if it be hence demonstrable that its greatest dilatation does barely amount to 5° 8′, and the perpendicular height of that part of the lower more condensed stratum of its atmosphere, which is capable of reflecting the solar rays, and of producing some other, perhaps more remarkable, obscurities in the stars seen through it, does not exceed 5000 Paris feet, and hence cannot reach above one second of a degree above the limb of the moon; we need not wonder that so small a magnitude, which loses itself in the inequalities of the limb, many parts of which are known to be considerably mountainous, should not become sensible, especially at the approach of a body of so large a diameter as Jupiter, and when so small a magnifying power is applied. And thus may I, with confidence, assert a perfect coincidence between this and my many other observations.

The appearance, fig. 5, when Jupiter, at the emersion, the objects being particularly sharp and distinct, came forth from behind the moon, which now covered no more than one-quarter of its diameter, was truly splendid and satisfactory; and I must here particularly mention the circumstance, that the part of the moon's dark hemisphere, between its bright terminating edge *m n*, and its outward limb, bordering upon the emerging planet, *o p*, was particularly opake, and hence produced a very striking effect.

I omit entering here upon any farther considerations, and shall conclude with observing that, after the occultation was completely ended, the luminous spot *l* had at 12^h 1′ so far advanced in its parallel *d e*, as to have reached to within $\frac{1}{6}$, or at most $\frac{1}{5}$, of its whole length, of the western limb. And that

Fig 1.

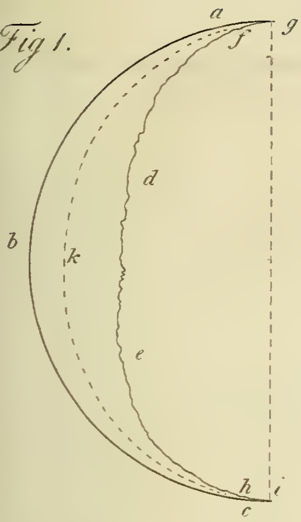


Fig 2.

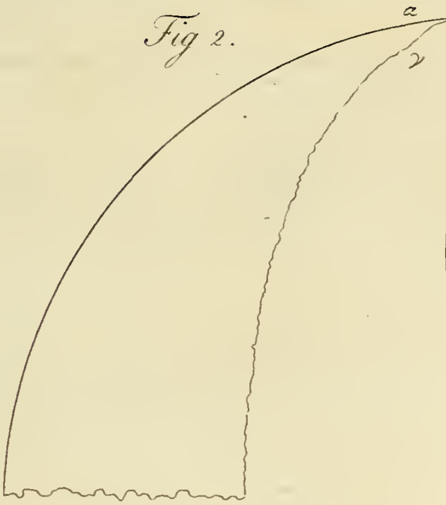


Fig 3.

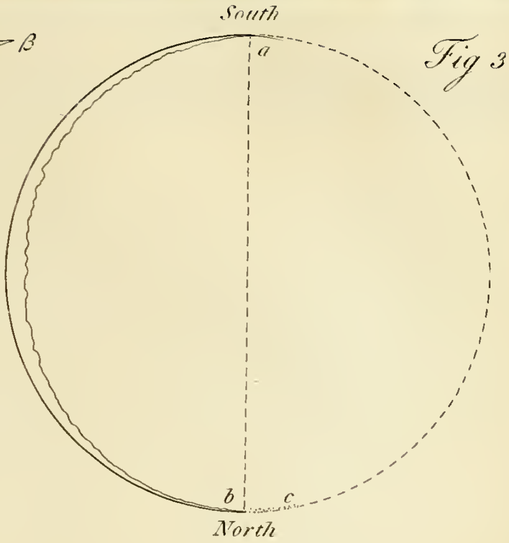


Fig 4.

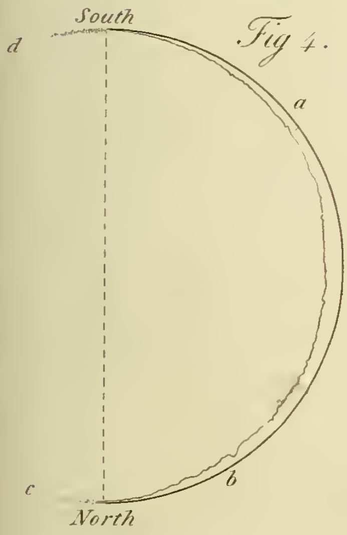


Fig 5.

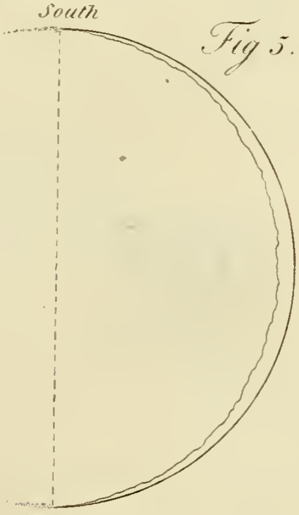


Fig 7.

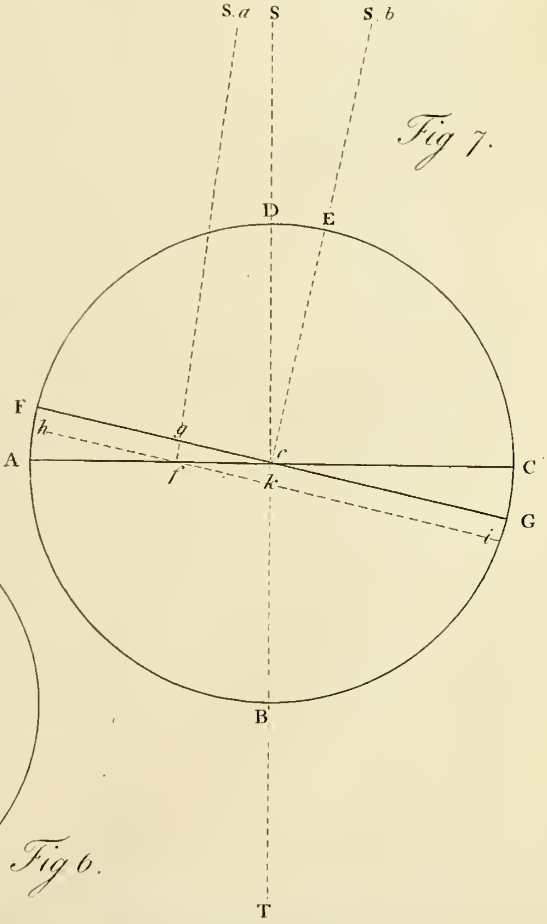
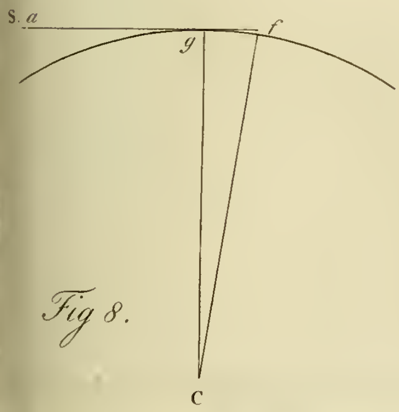


Fig 8.



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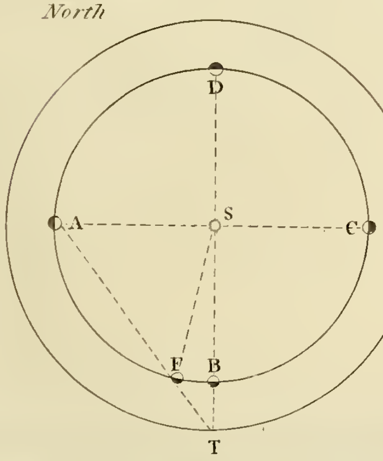
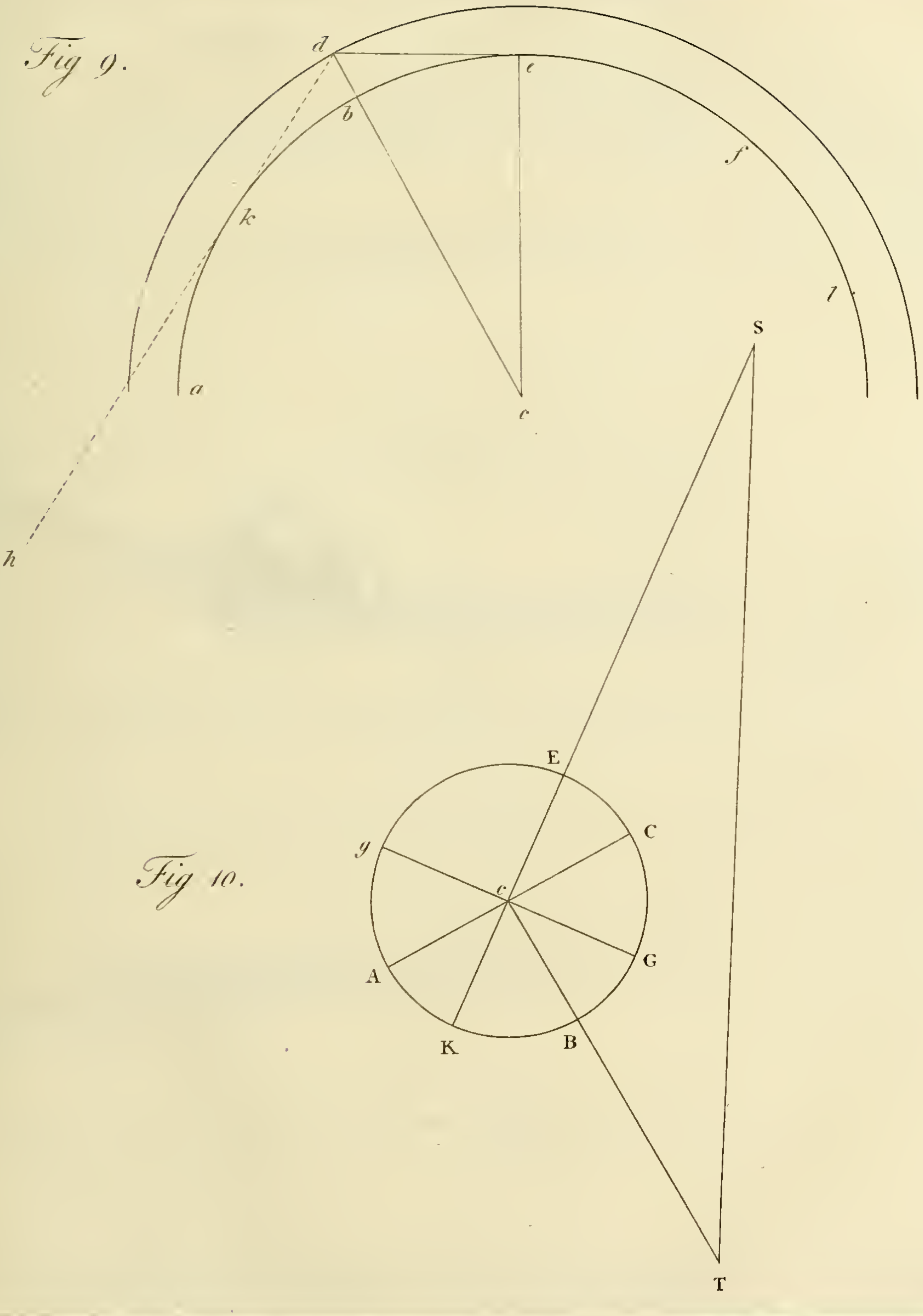


BRITISH MUSEUM
NATURAL HISTORY

Fig 9.

Fig 10.



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BRITISH MUSEUM
NATURAL HISTORY.

Fig 1.

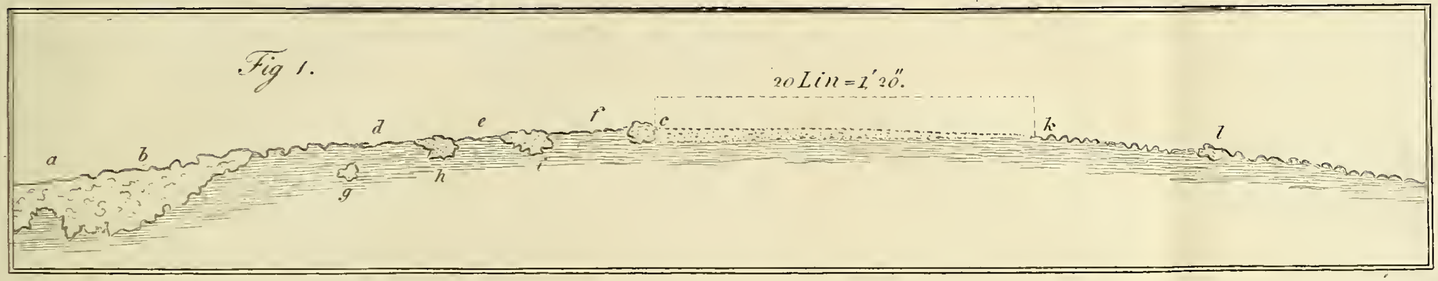


Fig 2.

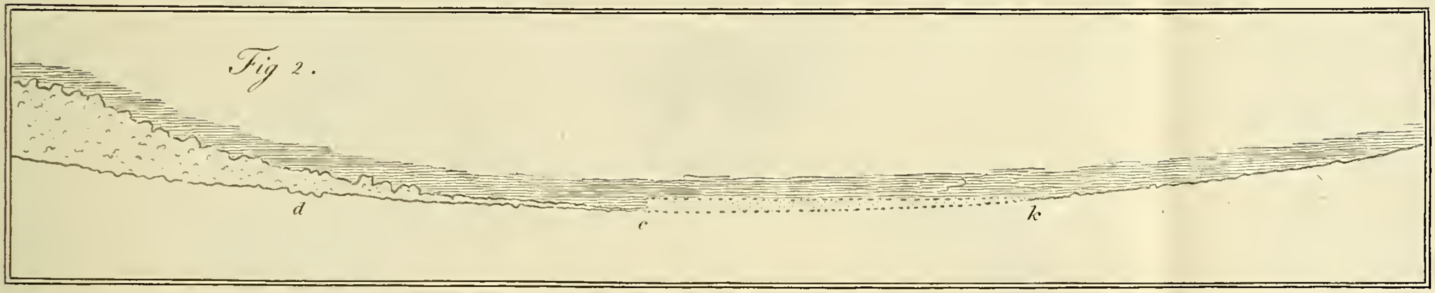
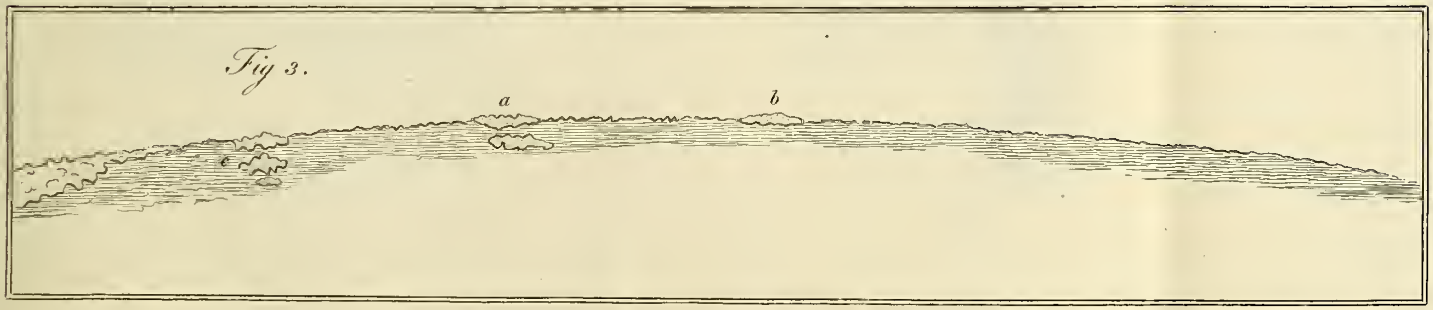


Fig 3.



BRITISH MUSEUM NATURAL HISTORY.

Fig 1.

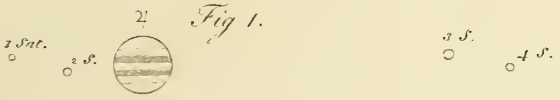


Fig 3.

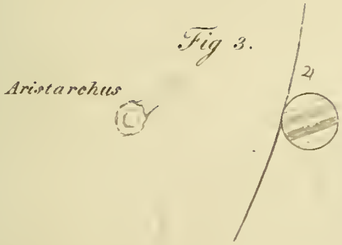


Fig 4.

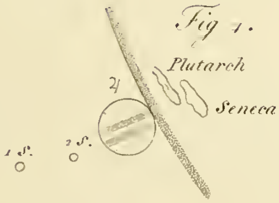


Fig 2.

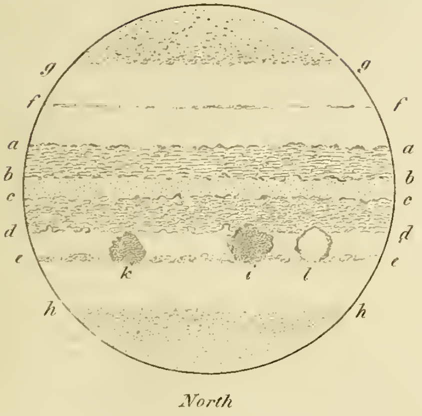
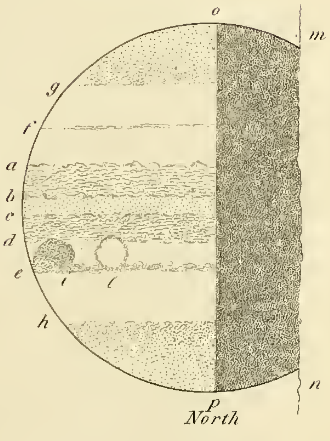


Fig 5.



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BRITISH MUSEUM
NATURAL HISTORY.

on the 28th of March, five days after a new moon, I observed an occultation of a very distinct, though telescopic star, by the dark hemisphere of the moon, in which, agreeably to the above observation, not the least gradual diminution of light, or indistinctness could be perceived, the star being seen to vanish on a sudden.

Lilienthal,
April 10, 1792.

XVII. *Abstract of a Register of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland. *By* Thomas Barker, *Esq.*; *with the Rain in* Surrey *and* Hampshire, *for the Year* 1791. *Communicated by* Thomas White, *Esq. F.R.S.*

Read June 14, 1792.

		Barometer.			Thermometer.						Rain.			
					In the House.			Abroad.						
		Highest.	Lowest.	Mean.	High.	Low.	Mean.	High.	Low.	Mean.	Lyndon.	South Lambeth.	Selbourn.	Fyfield.
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inches.	Inches.	Inches.	Inches.
Jan.	Morn.	29,92	27,92	29,01	44½	36	40½				2,410	2,91	6,73	3,82
	Aftern.				46	36	41							
Feb.	Morn.	96	28,66	43	48	37½	41½	48½	28½	37	1,268	2,29	4,64	1,81
	Aftern.				48	38	42½	51	35	42½				
Mar.	Morn.	30,11	41	67	51	38	45	49½	28	39	0,813	92	1,59	0,90
	Aftern.				53	40	46	56	37	47½				
Apr.	Morn.	29,65	62	30	56	46	50½	53½	39½	45	1,934	1,57	1,13	0,99½
	Aftern.				58	46½	52	65	45½	56				
May	Morn.	91	29,01	54	57	45	51	53½	38	47	1,140	76	1,33	0,59½
	Aftern.				59½	46	53	68	43½	57				
June	Morn.	78	06	49	67½	52	59	64	43	53½	0,921	60	0,91	0,71
	Aftern.				70½	53	61	80	52	66				
July	Morn.	80	00	40	66	56½	60	63½	51½	56	4,033	2,67	5,56	3,45½
	Aftern.				69	58	61½	79½	56	67				
Aug.	Morn.	30,06	02	59	69	55½	62	67	49	58	2,907	1,26	1,73	1,92
	Aftern.				71	57	63½	80½	58	68				
Sep.	Morn.	29,88	01	64	65½	53	58½	64	43	53	0,596	27	1,73	0,77
	Aftern.				69	54	60	72	55	63				
Oct.	Morn.	30,00	28,33	22	57½	44	50½	57	30½	43½	3,319	2,33	6,49	3,06½
	Aftern.				58½	45	51½	65	41½	52				
Nov.	Morn.	29,80	28,30	17	48½	38½	44	50	35	40	4,231	3,44	8,16	3,86½
	Aftern.				49	41	45	51½	36	44½				
Dec.	Morn.	88	50	14	44	29½	36	41	16	31	1,150	1,44	4,93	2,15
	Aftern.				44	29	37	46½	25½	35				
											24,722	20,46	44,93	24,05½

THE year began, as the last ended, with frost, but in a week's time the stormy wet weather returned, and, in some places, with thunder; but it gradually grew better, and by the middle of February there was a good deal of fine and pleasant weather, and, except a few frosty days and north winds the end of that month and beginning of March, the former part of spring was finer than usual; few NW winds, and those not so cold as they often are: chiefly dry in March; a good seed time, and a showery, warm, or almost hot, greater part of April, brought things on a great pace; and there was more grass at the end of that month than there sometimes is at the end of May.

But the two last days of April were colder; frequent frosty mornings, and cold NE winds in May, which being chiefly dry, the grass began to burn at the end of it; but there had been so much early grass that there was no want. The summer was a very uncertain one, sometimes very hot, at other times remarkably cold; and sharp frosty mornings the middle of June, with a little hail, and some said snow; killed, or near it, many tender summer plants; and there were very few natural ground cucumbers this year, but it was dry and burning at the end of the month. The weather was also very different in different countries: in the east and south-east parts of England they complained of a five-months drought; in this country, though the ground burned sometimes, as at the end of May, and the beginning and end of June, yet the grass had got so forward in April, that, with some rain at times, our ground was never bare; and fits of showery weather, and some great rains, made plenty of latter grass and turnips. At Selbourn, April, May, and June, were rather dry, but there was so much rain at the beginning and end of the year, and at the middle of it, in July, that there was a very great quantity upon the whole. The crop of hay was small, but well got; the wheat remarkably good, and barley proved better than was expected; other sorts of grain were moderate, and, in general, harvest was well got.

The latter part of summer and the autumn were very fine and pleasant, and sometimes hot; generally dry, so as rather tending to burn, the beginning of October; but before the middle it began to be wetter, and after the middle more stormy, and grew colder, with frosty mornings, cutting off the late flowers; and it continued wet the rest of October, and all November, and stormy in November and December; which last month was a very disagreeable one, for storms, snow, and frost, and sometimes all at once; and that almost the whole month, for a snow and severe frost, which began the third, though with several thawing days, was hardly quite gone by the end of the year: there was but little of this frost and snow in the south of England.

Of milking Ewes.

We find in the Greek and Roman writers, and also by some passages in the Bible, that it was the custom, in former times, to milk ewes as well as cows; and, perhaps, it may better be done in those hotter climates than in England; yet I find it spoken of as having been practised in this island many years ago. TUSSER, who lived in Suffolk, and was a young man in EDWARD the VIth's time (see his life at the end of his work), mentions it as used in his time, and he approved of doing so, as appears by the following quotations from his book, entitled, Five hundred Points of good Husbandry.

May's Abstract.

" Put lamb from ewe;
" To milk a few.
" Be not too bold
" To milk and to fold.
" Five ewes allow
" To every cow."

May's Husbandry.

" At PHILIP and JACOB away with the lambs,
" That thinkest to have any milk of their dams.
" At Lammas leave milking, for fear of a thing,
" Lest *requiem æternam* in winter they sing.
" To milk and to fold them is much to require,
" Except ye have pasture to fill their desire,
" Yet many by milking, such heed they do take,
" Not hurting their bodies, much profit do make.
" Five ewes to a cow, make proof by a score,
" Shall double thy dairy, else trust me no more.
" Yet may a good huswife, that knoweth the skill,
" Have mixt or unmixt at her pleasure and will."

He says, five ewes will give as much milk as one cow; and they used the milk sometimes mixed with cow's milk, and sometimes separate. Ewes should not be milked too freely, unless they have good pasture; but with prudent management, a profit may be made of milking them; and he is for doing it from May-day till Lammas, but not longer, for fear of killing them. As he seems to have been bred a singing boy at Wallingford, the *requiem æternam* probably refers to some hymn he there sang.

But, I suppose, this method of milking ewes was not found to answer upon the whole, for it is left off in England now. I never was in any part of it where it is practised at present; though by the increase of inclosure, and sowing of seeds, we have better pasture for our ewes than TUSSER had. I think it must be very bad for the lambs to be weaned at May-day, which is often before the grass is fully come, though his lambs were earlier than ours; for it appears in his January's Husbandry, that the ewes were about lambing then, whereas ours do not come till Lady-day. We do not wean our cade-lambs till June, in the height of summer, and then they are not so good as those which go with ewes till the latter end of August.

Note. The foregoing Register contained also some account of the earthquake of February 25; but as an account of it is already given by Mr. BARKER, in Mr. TURNOR's paper on the Earthquake, it is omitted here.

XVIII. *Observations on the remarkable Failure of Haddocks, on the Coasts of* Northumberland, Durham, *and* Yorkshire. *In two Letters from the Rev.* Cooper Abbs, *to Dr.* Blagden, *Sec. R. S.*

Read June 14, 1792.

Sunderland, May 9th, 1792.

SIR,

THE great loss sustained by the counties of Northumberland, Durham, and Yorkshire, by the almost total failure of Haddocks, during the three last seasons, is a circumstance of serious consequence to these maritime counties, and perhaps not unworthy the notice and attention of the gentleman and philosopher.

As far back as the memory of the oldest man reaches, for about three months in the year (beginning about Martinmas), prodigious quantities of haddocks, in fine weather, were daily caught on the above coasts, which gave employment to great numbers of men, and afforded a cheap and very acceptable article of food to all ranks of people, high and low. Besides the consumption on and near the coasts, great quantities were constantly carried at least a hundred miles into and over the country.

In the winter, 1789, I am very credibly informed, and sincerely believe, that not a ten-thousandth part (I speak much within bounds) of the usual quantity was taken; and, I can venture to say, the quantity has been not greater, if not much less, for the two last seasons, to the great astonishment of the poor fishermen.

I have frequently conversed with the most experienced men in this line of business, to discover, if possible, the cause of this extraordinary failure. One man, with more religious submission than philosophic judgment, ascribes it to the will and pleasure of the Almighty; a second, to the great quantities of ballast cast by the colliers into the sea, at or near the mouths of the rivers Tyne and Wear. But this seems a very inadequate reason; for granting this act might in some small degree affect these places for a few miles, yet it could not affect the coasts at any considerable distance, either to the north or south. This last circumstance has in some degree affected the lobsters within a few miles of the two rivers, by filling up the holes and cavities in or under the rocks, where the lobsters used formerly to lie, and retreat to in stormy weather; so that being now in a great measure deprived of their old abodes of security, they are frequently, in storms and tempests, thrown on the shore, shattered and broken in pieces. A third ascribes the failure to the great number of dogfish on the coasts; but I suppose the number of them to be nearly the same, year by year. The dogfish is very voracious, and a great enemy to the fisherman and his tackle, and therefore never spared when caught: besides, it is well known that dogfish chiefly pursue the shoals of herrings, which have left these coasts before the haddocks come on. A fourth says, the shoal of haddocks has met with beds of copperas at the bottom of the sea, and thereby is poisoned ; but why should such beds (supposing the case true) have worse effects in 1789, than at any time before?

It is an indisputable fact, that many ships, on the return from Archangel, in the latter end of 1789, saw immense quantities of haddocks (no other fish were particularized), for fifty or sixty leagues, I believe, lying dead on the surface of the sea, but could not at that time ascribe any cause for the event. I believe about that time an eruption broke out in Hecla, and perhaps it may with some degree of probability be conjectured, that volcanic matter, of noxious quality, may have burst in the sea, and occasioned the above destruction and failure ever since.

The few haddocks caught in 1789 and 1790, were remarkably large; these keep nearest the shore: the small ones lie more out to sea ; so that, when fishermen were wont to catch small haddocks, they desisted, and came nearer the shore to procure the large ones. The shoal generally lay about one league from the shore, was about three miles in breadth, and in length extended near the whole coasts of the three counties, in constant succession, for about three months. The breed of haddocks seems nearly destroyed on these coasts, which is a loss of many thousands of pounds *per annum* to fishermen and others, besides the loss of a very plentiful and acceptable article of food to persons of all ranks, especially in the winter season, when the price of provisions bears hard upon the poor.

May I hazard one question: Is it probable that, in the ensuing winter, or a few succeeding ones, the fishery may recover by the return of a shoal of haddocks? For the two last winters I have waited with anxiety, but in vain, for such an event to take place.

I am, &c.

COOPER ABBS.

Sunderland, May 27, 1792.

SIR,

Encouraged by the unexpected and favourable attention paid to my letter by Sir Joseph Banks and yourself, I have, since the receipt of your favour of the 19th, daily mounted my horse, to endeavour to gain further information on the mysterious subject. Three days ago, I was fortunate enough to hear of two persons in Northumberland who were at Archangel in 1789, and waited upon them yesterday. As they lived about two miles asunder from each other, the one at North Shields, and the other at a village in the country, I had an opportunity of hearing, and asking them questions separately. Their names are, Mr. John Stoker, of the Ranger, and Mr. John Armstrong, of the Integrity, of North Shields, masters of ships of considerable size and value, men of sober, decent character, intelligent and respected in their line of profession, from whom I received the following account, which I have every reason to believe true. That in the latter end of July, 1789, on the light passage to Archangel, after doubling the North Cape (where they joined eight or ten sail of large ships from various ports and nations), and reducing their latitude from 69 to 68, between Fisher's Island and Sweetnose, for about thirty leagues east and south, they, to their great surprise, for the space of three days, in which they had variable winds, or light airs, fell in with immense quantities of haddocks and coalfish, and no others whatever (the last is an ordinary fish, and perhaps its name may be local here, and unknown to you, but I confess I do not know any other name it has), lying on the surface of the ocean, and sufficient, from the view they had of them for the three days, to have loaded all the ships then in company. That they found them for the space of between twenty and thirty leagues in length, and in breadth, to the east, from three to five leagues, as the ships stood off and on; but how much farther to the east, and a few other points, they might extend, these persons cannot pretend to say, such points being out of their course for the ports they were destined to. That most of the fish were dead, though some were alive, as appeared by a slight motion of the tail, but in a very weak, feeble state, and unable to sink in the water.

In the above particulars Messrs. STOKER and ARMSTRONG perfectly agree, as to the truth of the fact. The latter, through cautious timidity, prevented his crew from taking up any of the fish; but the former took on board many, both dead and in a dying state, of which he first ate, and then suffered his men to do the same: and at Archangel gave the remainder to the customhouse officers, without any person receiving the least injury. Mr. STOKER having, previous to eating the fish, tried the usual experiment at sea, of putting silver into the fresh water wherein the fish were boiled, the silver was not at all discoloured.

Talking with Mr. STOKER, in his parlour, I asked him how many fish he could take up in that or any other given space. He answered, that in various places the fish lay so thick, that in the compass of twelve or fifteen yards a boat load, from three to five tons, might have been taken up: that he measured several of the haddocks, from two to two feet eight inches in length, and six or seven inches deep; about twice the size of haddocks on our coasts. That he opened all the haddocks he took on board, and in every one of them, both dead and expiring, he saw the *sound* much inflated or blown up, to which he ascribes the great destruction, but without being able to give any further satisfactory reason.

Mr. STOKER went from Archangel to Onega; and when Mr. ARMSTRONG, at the former place, related the story to the merchants and inhabitants at the Exchange, they replied, that they had known and heard of similar accidents; and that the great quantity of thunder and lightning, usual near the Cape, was the reason.

To the above relation, which I verily believe to be just and true, I shall not presume to add a word of my own, but submit the whole to your consideration.

In my excursion along the coast of Northumberland, I found a fisherman careening his boat, who told me that, prior to the late failure, he had frequently, with the assistance of two men, taken and sent to Newcastle, in one day, two boat loads of haddocks, containing in each from eighty to a hundred score; but in the last season he had not, in the whole, taken more than forty or fifty haddocks. He could give no reason for the failure, but another man attributed the scarcity to the want of hard gales of wind, for some years, to blow the fish off the Dogger Bank to these coasts.

The accounts received from Messrs. STOKER and ARMSTRONG being more authentic than what I had heard before, will be a sufficient apology, I trust, for the few slight variations in my two letters.

I verily believe I cannot trace this subject further, without able instructions from gentlemen well versed in a branch of knowledge wherein I am a novice.

I am, &c.

COOPER ABBS.

XIX. *On the Cause of the additional Weight which Metals acquire by being calcined. In a Letter from* George Fordyce, *M.D. F.R.S. to Sir* Joseph Banks, *Bart. P.R.S.*

Read June, 21, 1792.

SIR,

ALTHOUGH many chemists are at present satisfied of the nonentity of what was formerly supposed to be a body, called phlogiston, and considered as an element contained in metals when in their metallic form, yet this supposition has interwoven itself so much into chemistry in general, and has been so universally received, that it may not be superfluous to relate the following experiments. If you are of that opinion, I shall be obliged to you if you will lay them before the Royal Society.

When a man begins to make an experiment, however well digested his plan may be, he finds, when he comes to put it in execution, that he must make a great number of experiments before he can bring the fact to be proved, or disproved, fairly in issue; if I may take a phrase from law, there are first many buts and rebuts. It has often been the practice to bring all this previous matter before the public. This practice may be very proper, although it does not seem so to me, and therefore I have not troubled the Society with the difficulties and disappointments I have met with; but have brought the simple experiments forward in such manner as that they can be easily repeated by any person who is at all versed in chemistry, and possesses those most necessary qualifications in this science, patience and accuracy. I am, Sir,

Your most obliged humble servant,

G. FORDYCE.

It has been a great desideratum among chemists to determine the cause of the additional weight which metals acquire when they are calcined. To investigate this subject, I had begun the following experiment many years ago, but various other engagements have so much interrupted me, that I have had but little time to pursue any other chemical inquiry than such as were necessary to form the catalogue of the ores and minerals in Dr. Hunter's museum.

There is great difficulty in choosing the metal on which inquiry should be instituted, on account of the differences of their calces. After a number of trials, I chose zinc, as that whose calces appeared to differ the least from one another; in other respects there are great objections to it likewise, but which may be got over.

I took a portion of the zinc I employed, and dissolved it in vitriolic acid, with which it made a clear solution (without any of that black matter which commonly separates during its solution when we employ zinc imported from abroad). After precipitating it by an alkali, and exposing the calx to the air, it remained of a pure white; so that it could contain no iron. This zinc was reduced to its perfect metallic form by breaking it into small particles, and melting it with black flux, taking that part of it only which was at the bottom of the crucible.

I reduced this metal to a calx, by dissolving it in vitriolic acid diluted with water, and precipitated it by *kali purum* dissolved in water.

In doing this, the acid should be diluted with four or five times its weight of water, and the zinc should be dissolved very slowly, avoiding heat as much as possible during the solution. If this precaution is not taken, a quantity of volatile vitriolic acid will be produced, and spoil the experiment.

In the precipitation the alkali is apt to re-dissolve the calx, if care be not taken to use it in solution in water, and that the solution is diluted with a large quantity of water: the proportion in which the water is in *aqua kali puri* of the London Dispensatory is a convenient solution of the alkali.

Care must likewise be taken, in the precipitation, that the solution of the kali be poured into the solution of the *zincum vitriolatum* in water by a little at a time, and that the whole be perfectly mixed together before a fresh quantity is poured in, otherwise part of the calx will be re-dissolved. It is farther necessary that the exact quantity of *kali purum* be used: if too little is used, the whole calx will not be separated; if too much, part of the calx will be re-dissolved. It is also necessary that the alkali be perfectly pure, especially free from fixed air,* as that would be transferred to the calx, and as it flies off when the kali is simply united with vitriolic acid, the accuracy of the experiment would be thus destroyed.

The weight of the calx, by which it exceeds the weight of the metal, shews that there is a substance added to the whole metal; or, that while some substance is driven off, another is added in greater quantity; since it is clear, from various experiments well known to this learned body, that all matter gravitates, and that all the substances found in this earth, which have been tried, gravitate equally. This additional matter must be added to the metal either from the acid, the alkali, the water used in the solution, the air lying on the surface of the materials during the time of the operation, or it must come through the vessels in which the operation is performed. To ascertain this, I made the following experiment.

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* I use the name of fixed air, although certainly not proper, in order to avoid running into confusion by employing those which have been given to this substance, until the plurality of voices shall fix an appropriated name to it.

I took a large quantity of vitriolic acid, purified by distillation (about two pounds, it not being material what quantity was taken exactly), I diluted it with distilled water about four or five times its weight by guess (the exact proportion being also immaterial), I applied to 1000 grains of this diluted acid a sufficient quantity for saturation of *aqua kali puri*, of the London Dispensatory, rendered pure from fixed air, as is prescribed in the process of the College; I poured in the *aqua kali puri* to the diluted acid, by a little at a time, until it was nearly saturated. I then poured in some juice of violets, which gave the whole a red colour. I continued to add *aqua kali puri*, by a little at a time, until the red colour just disappeared. I added the *aqua kali puri* to the acid, rather than the acid to the alkali, because the loss of the red colour at the point of saturation can be discerned much better than the loss of the yellow colour, which the alkali intermixes with the natural blue.

I ascertained the weight of the *aqua kali puri*, by weighing the bottle containing it before any was poured into the acid, and after the saturation took place; the deficiency of weight afterwards being the weight of the *aqua kali puri* applied to the acid for the saturation; this was 10147 grains. I also weighed the vessel with the acid before the *aqua kali puri* was poured in, and afterwards; and found the increase of weight to be exactly the same as the weight of the *aqua kali puri* and juice of violets, so that nothing was lost during the operation.

This experiment was three times repeated, taking the point of saturation from the eye. The quantities of *aqua kali puri* employed were found to be 10147 grains, 10145 grains, 10150 grains.

I took 10148 grains, being the mean of the three experiments, and applied it to 1000 grains of the same vitriolic acid; evaporated the water to dryness, and heated it to a red heat, to drive off the whole of the water; and found 978 grains of *kali vitriolatum* remaining. By this means I ascertained the quantity of *kali vitriolatum* produced from 1000 grains of the diluted vitriolic acid, when saturated with kali.

I took 1000 grains of the diluted vitriolic acid, and put it into a vessel, of the form in the figure annexed (Tab. X.), I added zinc to it until it would dissolve no more; I caught, during the solution, the inflammable air, which weighed 9 grains, and whose specific gravity was, to atmospheric air, as somewhat less than 1 to 12. The vessel contained the whole of the acid and the zinc in the globular part marked A, the acid being introduced by a funnel.

The solution was terminated in five days; when part of the tube D being broke off, it was left to stand for four-and-twenty hours, to allow the inflammable air remaining in the vessel to fly off, and give place to the air of the atmosphere; which happened spontaneously from the different specific gravities of the two vapours.

The vessel containing the solution of the zinc was now laid upon its side, and 10148 grains of *aqua kali puri* were introduced by a crooked funnel into the globe B, being the quantity sufficient to saturate 1000 grains of vitriolic acid, as before determined. Then the tube D was hermetically sealed, and the whole weighed. The vessel was then raised, so that the globe A was undermost; this was done very gradually, so that the *aqua kali puri* was gradually added to the solution of the zinc: when a little was poured in, the vessel was brought into an horizontal position again, and shaken a little; this was repeated until the whole of the *aqua kali puri* was poured in. The zinc was thus precipitated in the form of a calx. It was suffered to stand for forty-eight hours: no alteration of the gravity took place, therefore nothing had entered through the glass to give additional weight to the zinc in order to calcine it.

The next step was to open the tube, which was done under water, and in an atmosphere of the same heat in which it was sealed, to wit, 57° of FAHRENHEIT's thermometer. The air was neither diminished nor increased, none of the water being driven into the apparatus by the weight of the atmosphere, and none being thrown out. On heating the globe B, so as to drive out some of the air, it was found to be of the same purity, nearly, as that of the atmosphere, being tried by the application of nitrous air produced from solution of mercury.

The weight, therefore, which the calx had gained, arose neither from any substance passing through the glass, nor ftom the super-incumbent air during the precipitation. It must, therefore, be either from the acid, the alkali, or the water.

To determine whether the acid or alkali gave the weight to the calx of the zinc, I washed out the *kali vitriolatum*, formed by the combination of the vitriolic acid and the kali, with pure water, repeatedly applied, until it came away as pure as when applied, to all sensible trials. The quantity of water used was above four pounds. I evaporated this water to dryness, and heated the mass red hot, to expel the whole of the water; it weighed seven grains more than the vitriolated tartar procured from applying the acid and alkali as above. After evaporating the water, I dissolved the mass again in 40 ounces Troy weight of pure water; a yellowish powder separated. The solution of the vitriolated tartar, cleared of this powder, was again evaporated to dryness, and the water of crystallization driven off. It now weighed $976\frac{1}{16}$ grains, which is nearly two grains less than the vitriolated tartar I obtained from the acid and alkali applied simply together, without the intervention of the zinc.

The vitriolated tartar now obtained was free from any mixture. The additional weight of the calx of the zinc did not arise, therefore, from either the acid or the alkali: it remains, therefore, that it arose from the water.

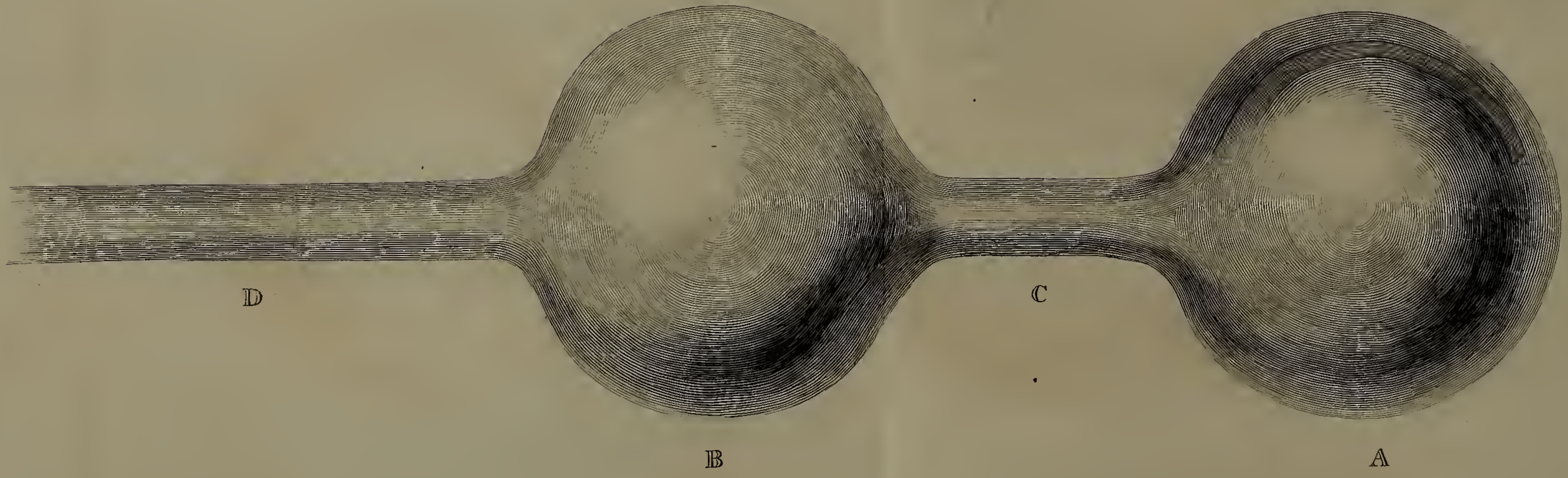
The weight of the calx of the zinc was ascertained by drying it after washing out the vitriolated tartar, heating it to a red heat, and afterwards weighing it. The weight of the zinc dissolved in saturating the acid, was 164 grains: the weight of the calx 220 grains. The additional weight was, therefore, 56 grains.

If it arose from the water, then a quantity of water, equal to the weight by which the calx exceeds the metal, must be lost in the operation. To determine this, I performed a distillation in the following manner.

I put 1000 grains of the same diluted vitriolic acid into the globe A of the same apparatus, then introduced the quantity of *aqua kali puri* found necessary to saturate it. The tube D was then bent downwards about the middle, and the apparatus brought to an horizontal position; so that the bent part of the tube was in a perpendicular direction downwards: to this I affixed a small phial, and weighed the whole. I then put the globe B in a box filled with ice, and applied heat to the globe A, so as to distil over the water into the globe B, the liquor never being brought to the boiling point. When the matter in the globe A became dry, the heat was increased to a red one, to distil over likewise the water of crystallization. The whole apparatus was now weighed, and found not to have lost a grain; nor was there any water in the phial. I then cracked the tube C, by applying a red hot iron to it, and letting a drop of cold water fall upon it. I next weighed the globe B with the water in it, then poured out the water, and let the glass dry. I weighed the glass; the deficient weight from the former weighing, being the weight of the water, was 10098 grains.

I repeated the experiment, with this difference; I put 1000 grains of the same vitriolic acid into the globe A, then introduced the quantity of zinc sufficient to saturate it: I took the weight of the inflammable air as before, and found it nearly the same in weight and quality. The same quantity of *aqua kali puri* was then introduced through a funnel as in the former experiment, then the tube was bent downwards, and a phial applied to it as before. The whole apparatus was weighed after the distillation, and found not to have lost any sensible quantity of weight, nor was there any water in the phial. The phial being detached, and the tube broken as before, the globe weighed again when dry, the deficiency was less than in the former experiment by 63 grains, which is two grains less than the additional weight of the calx above the metal and of the inflammable air taken together; and therefore the matter occasioning the additional weight of the calx above that of the metal, and the inflammable air, are both produced from the water.

Philos. Trans. MDCCXCII. *Tab.* X. *p.* 382.



Baſire ſc.

XX. *On the Civil Year of the* Hindoos, *and its Divisions; with an Account of three* Hindoo *Almanacs belonging to* Charles Wilkins, *Esq.* *By* Henry Cavendish, *Esq.*

Read June 21, 1792.

Though we have received much information concerning the astronomy of the Hindoos, we know but little of their civil year, and its divisions; and what accounts of it we have received vary much from each other, owing partly, as will be seen, to different methods being used in different parts of India. As it occurred to me, that the best way by which a person in Europe could clear up the difficulties in this subject, would be to examine the *patras*, or almanacs, published by the Hindoos themselves, I applied to Mr. Wilkins, well known for his skill in the Sanskreet language, who was so good as to lend me three such, and assist me in finding out their meaning.

One of them was procured by Mr. Wilkins at Benares, and is computed for that place. The second came from Tanna, in the island of Salsette, near Bombay; but it appears to be the copy of a Benares patra, as it is disposed in the same form as the first, and is adapted to the same latitude and longitude. The third is computed for Nadeea, a town of Bengal, about 50 miles N. of Calcutta, almost as noted for learned men as Benares, and much frequented by students from the coast of

Coromandel. The language of all three of them is a corrupt Sanskreet; but the last is written in the common Bengal character.

It appears from these almanacs that the civil year is regulated very differently in different parts of India; but before I speak of this year, it will be proper to mention a few words of the astronomical, which in all parts serves to regulate the civil year.

The astronomical year begins at the instant when the sun comes to the first point of the Hindoo zodiac. In the present year, 1792, it began, according to the principles delivered in the *Surya Siddhanta*,* on April 9, at 22^h 14′ after midnight of their first meridian, which is about 41′ of time west of Calcutta; but according to Mr. GENTIL's account of the Indian astronomy, it began 3^h 24′ earlier. As this year, however, is longer than ours, its commencement falls continually later in respect of the Julian year by 50′ 26″ in four years.

This year is divided into 12 months, each of which corresponds to the time of the sun's stay in some sign, so that they are of different lengths, and seldom begin at the beginning of a day.

The civil day, in all parts of India, begins at sun-rise, and is divided into 60 parts, called dandas, which are again divided into 60 palas.

The only parts of the Benares patras which are of any material use for my purpose, are the names of the months which are set down at the top of each page, and the three first columns, the first of which contains the day of the month, according to the civil account, the next the day of the week, and the third the time at which the lunar *teethee* ends; but as many may like to be informed of the nature of an Hindoo almanac, I shall give an account of the remaining parts at the end of this paper.

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* See an account of this in the 2d volume of the Asiatic Researches.

In those parts of India in which this almanac is used, the civil year is lunisolar, consisting of 12 lunar months, with an intercalary month inserted between them occasionally. It begins at the day after the new moon next before the beginning of the solar year.*

The lunar month is divided into thirty parts, called teethees; these are not strictly of the same length, but are equal to the time in which the moon's true motion from the sun is 12°. From the new moon till the moon arrives at 12° distance from the sun, is called the first teethee. From thence till it comes to 24°, is called the second teethee; and so on till the full moon; after which the teethees return in the same order as before.

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* My reasons for saying that the civil year begins at the day after the new moon next before the beginning of the solar year, are as follow: 1st. These almanacs begin at this time, and, moreover, the year of Veekramādeetya and Sālavāhana, which is set down at the top of each page, is the same in the first page as in all the following, which would be improper, unless the year began at this time. 2dly. In the calculation of the eclipse of the sun, in PERE PATOUILLET's Memoir, given in BAILLY's *Astronomie Indienne,* the computation is made for the new moon preceding the beginning of the solar year, and yet the year of Sālavāhana, and of the cycle of 60, set down in the Memoir, is the same as if the solar year was already begun. 3dly. PERE DU CHAMP, in his table of the names of the years of the cycle of 60, given in the same book, has added to some of them the corresponding year of Christ, together with a day of the month. This day, in all of them, is the day next after the new moon, preceding the beginning of the solar year: and though no explanation is given, must evidently be intended for the day on which the year begins. And, 4thly. It is said in the *Ayeen Akbery,* by ABRAHAM ROGER, and, I believe, some other authors, that the year begins at this time. To the three last authorities, indeed, it may be objected, that they are taken from places in which we do not know that the Benares almanac is used; but they shew, that in some parts of India the year begins at that time, and if it does so in any place, it most likely does at Benares.

The civil day is constantly called by the number of that teethee which expires during the course of the day.

As the teethee is sometimes longer than one day, a day sometimes occurs in which no teethee ends. When this is the case, the day is called by the same number as the following day; so that two successive days go by the same name.

It oftener happens that two teethees end on the same day, in which case the number of the first of them gives name to the day, and there is no day called by the number of the last; so that a gap is made in the order of the days.

In the latter part of the month the days are counted from the full moon, in the same manner as in the former part they are counted from the new moon; only the last day, or that on which the new moon happens, is called the 30th instead of the 15th.

It follows from what has been said, that each half of the month constantly begins on the day after that on which the new or full moon falls; only sometimes the half month begins with the second day, the first being wanting.

The manner of counting the days, as we have seen, is sufficiently intricate; but that of counting the months, is still more so.

The civil year, as was before said, begins at the day after the new moon; and moreover, in the years which have an intercalary month, this month begins at the day after the new moon; but notwithstanding this, the ordinary civil month begins at the day after the full moon. To make their method more intelligible, I will call the time from new moon to new moon, the natural month. The civil month Visākha begins at the day after the full moon of that natural month which commences at the beginning of the civil year, or, in other words, at the day after the full moon of that natural month during which the sun enters the first Hindoo sign. Jyēshtha begins on the day after the full moon of that natural month during which the sun enters the second sign, and so on. The names of the civil months, with the names of the signs which the sun enters during the natural month at the full moon of which the civil month begins, are given in the following table, to which I have also added the day of our month when the sun entered that sign in the latter part of the year 1784, and beginning of 1785, taken from the Benares almanac, the time of the day being counted from sun-rise, and expressed in the Hindoo manner.

Civil Month.	Sign.	Day on which the ⊙ enters it.			
		1784.	day.	dan.	pa.
Visākha	Mesha	April	9,	37,	7
Jyēshtha	Vreesha	May	10,	34,	8
Āshāra	Meetoona	June	11,	0,	8
Srāvana	Karkata	July	12,	37,	58
Bhādra	Seengha	August	13,	7,	11
Aswēena	Kanyā	Sept.	13,	7,	36
Kārteeka	Toolā	Octob.	13,	32,	55
Mārgaseersha	Vreescheeka	Nov.	12,	25,	38
Powsha	Dhanoo	Decem.	11,	54,	18
		1785.			
Māgha	Makara	Jan.	10,	13,	11
Phālgoona	Koombha	Feb.	8,	40,	21
Chitra	Meena	March	10,	30,	38

It may be observed, that in general, Visākha begins at the day after that full moon which is nearest to the instant at which the sun enters Mesha, whether before or after; however, it is not always accurately the nearest.

The two parts of each month are distinguished in these almanacs by the addition of the syllables *vadee* and *soodha* to the name; thus the first half of Visākha, or that from the day after the full, to the day after the new moon is called Visākha-vadee, and the remainder Visākha-soodha*; but, I believe, the more usual way of distinguishing them is by the words *kreeshna paksha*, or the dark side, and *sookla paksha*, the bright side.

A consequence of this way of counting the months is, that the first half of Chitra falls in one year, and the latter half in the following year.

Whenever the sun enters no sign during a natural month, this month is intercalary, and makes an irregularity, which may best be explained by an example.

In the year 1779, the sun entered into no sign during the natural month which began at the end of the first fortnight of Srāvana; accordingly the whole of this month was intercalary, and the fortnight which preceded it was called Neeja Srāvana vadee, instead of simply Srāvana vadee, as it would otherwise have been named. The first half of the intercalary month was called Adheeka Srāvana soodha, and the latter half Adheeka Srāvana vadee, and the fortnight after the intercalary month, Neeja Srāvana soodha.†

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* *Soodha* signifies clear, pure, or complete; but the word *Vadee* is not to be found in any of Mr. WILKINS's dictionaries.

† *Adheeka* signifies over and above, or intercalary. *Neeja* prefixed to the name of the month signifies that month itself.

It appears, therefore, that the two parts of the month where the intercalation takes place, are separated from each other by the interval of the whole intercalary month, and have the word Neeja prefixed to them; and the two parts of the intercalary month are called by the same name, but have the word Adheeka prefixed.*

In these almanacs no notice is taken of the solar months, notwithstanding that a column is allotted to the day of the Mahometan calendar, which seems to shew that, in the countries which use the Benares patra, it is not customary to date by the solar month; for it is very unlikely that the computers of these almanacs should have given the days of the Mahometan calendar, and yet have omitted days used in their own.

In those parts of India which use the Nadeea patra, the case is quite different. This almanac contains the name of the solar and lunar month, with the corresponding days of the week and solar month, and the number of the lunar teethee which ends on those days. It begins with the day after that on which the astronomical year commences. This is marked as the first of the month, the next day is called the second, and so on, regularly to the end of the month. In like manner, all the other months begin on the day after the astronomical commencement, and the days are continued regularly to the end, so that the number of days in the month varies from 29 to 32.*

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* What has been here said, agrees perfectly with Mr. WILKINS's almanacs; the only doubt is, whether there may not be some different method of regulating the month, which may also agree with these almanacs, and may be the true one. It is proper, therefore, that I should state my reasons for the account here given. DU CHAMP, who seems a very accurate writer, says (see BAILLY, p. 320) that he was informed by a Hindoo calculator, that whenever the sun enters no sign during a lunar month, that month is doubled. This passage agrees very well with these almanacs, if by month we mean the time between two new moons; but disagrees entirely with them if we mean by it the time between two full moons; and moreover, in Mr. WILKINS's almanac it is the period from one new moon to another, which is called Adheeka. It seems certain, therefore, that in this passage the word month must mean what I have called the natural month; and that the rule for intercalation is such as I have mentioned, namely, that it shall take place whenever the sun enters no sign during the natural month. It is certain also that the ordinary civil month begins at the day after the full moon; and granting these two points, I cannot see any way in which the months can be regulated so as to differ in substance from what I have said.

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* Perhaps I do not express myself accurately in saying that the civil month begins at the day after the commencement of the astronomical. It is true, that in this almanac it is the day after the commencement of the astronomical month, which is marked by the number one; but it must be observed that the Hindoos count by years complete, not by years current: for example, the year 1000 of the Kalee Yug begins at the time when 1000 years are completed from the Kalee Yug; and it is likely that the same manner of counting is adopted with regard to days, so that the day of the month marked one, does not signify the first day, but the day which begins at the expiration of the first day, and consequently that the civil month begins at the sun-rise of the day on which the astronomical month begins. I, however, have chosen to say that it begins at the day after, partly because I am not sure that the foregoing is the true meaning of the Hindoos, and partly because it would have been difficult to express myself in such manner as not to run great risk of being misunderstood, if I had done otherwise. What is here said applies equally to the lunar month in this and the Benares almanacs.

Though it is foreign to the subject of this paper, I cannot refrain from taking notice of an error, which I apprehend many European astronomers have fallen into, from not distinguishing between days current and days complete. It is common to say that the astronomical day begins twelve hours later than the civil day, and the nautical day twelve hours sooner; and it is true that the hour, which, according to the civil account is called one in the afternoon of the first of January, is written by astronomers January $1^d\ 1^h$, but this, I apprehend, ought not to be read 1^h on the 1st of January, but 1^d and 1^h from the beginning of January, so that in reality the astronomical and nautical day both begin 12^h before the civil. A proof of the truth of this is, that in astronomical tables the place of the heavenly bodies set down for the beginning of the year, is the place for noon of the last civil day of the preceding year; and moreover, in HALLEY's tables this place is said to be *annis Julianis ineuntibus*, which shews that he thought that this was not meely a practice used for the sake of convenience, but that the year actually begins at this time.

The names of the months are the same as those of the lunar months in the Benares patra, Visākha being the first, or that which corresponds with the sign Mesha.

The lunar months begin, not at the full, as in the Benares patra, but at the new moon, and are called by the name of that solar month which ends during the course of them; for example, the lunar month, during which the solar month Visākha ends, is called Chandra (or lunar) Visākha, so that each month begins a fortnight later than by the Benares patra.

The teethees do not recommence at the full moon, but are continued to the end of the month, or to the 30th. In other respects they are counted as in the Benares patra; that is, the same notation is used whenever a day occurs in which no teethee ends, or when two teethees end on the same day.

Unluckily no intercalary month occurred in the year for which this almanac was computed, so that it gives us no information about the method of intercalation; but from analogy we may conclude, that those lunar months in which the sun enters no sign are intercalary, and are called by the name of either the preceding or following month, with the addition of some word to denote that they are intercalary.*

As the Nadeea almanac begins with the day after the commencement of the solar year, and gives the day of the solar month, which the Benares patra does not, it affords reason to think that the custom of that part of India in which it is used, is to date by the solar month, and begin the year on the next day to the astronomical year; and accordingly Mr. WILKINS informs me, that the Hindoos of Bengal, in all their common transactions, date according to solar time, and use what is commonly called the Bengal era, but in the correspondence of the Brahmins, dating books, and regulating feasts and fasts, they generally note the teethee; and if the year is mentioned, it is often that of Veekramādeetya, sometimes that of Sālavāhana, but more frequently the vulgar Bengal year.

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* The Chinese, who, like the Hindoos, consider that lunar month as intercalary in which the sun enters no sign, call it by the same name as the preceding month; and it is likely that the Bengalese do so too.

From what has been said, it appears, that the Hindoo civil months, both solar and lunar, consist, neither of a determinate number of days, nor are regulated by any cycle, but depend solely on the motions of the sun and moon, so that a Hindoo has no way of knowing what day of the month it is, but by consulting his almanac; and what is more, the month ought sometimes to begin on different days, in different places, on account of the difference in latitude and longitude, not to mention the difference which may arise from errors in computation. The inconvenience with which this must be attended seemed so great to me, that two or three years ago, by the assistance of Sir JOSEPH BANKS, I proposed a query on the subject to Mr. DAVIS, author of the very valuable paper, in the Asiatic Researches, on the Hindoo astronomy, inquiring whether any method was taken to avoid the ambiguity, and was favoured with the following answer.

" My Pundit, and others with whom I have conversed on the " subject, although well aware of the circumstance (that the " month may begin on different days in different places) do " not think the ambiguity thence arising of much consequence, " nor is there any method they know of taken to avoid it.

" The almanacs in common use are computed at Benares, " Tirhut,* and Nadeea, the three principal seminaries of " Hindoo learning in the Company's provinces, whence they " are annually dispersed throughout the adjacent country. " Every Brahmin in charge of a temple, or whose duty it is " to announce the times for the observance of religious ceremonies, is furnished with one of these almanacs ; and if he " be an astronomer, he makes such corrections in it as the " difference of latitude and longitude render necessary.

" The beginning of the solar month falling on different " days of the week, is not, as I have observed, regarded ; " but a disagreement in the computation of the teethee, " which sometimes also happens, occasions no small perplexity, " because by the teethees, or lunar days, are regulated most " of their religious festivals : and I am assured that an instance of this kind, which occurred in Cossim Ally's time, " obliged the Rajah of Nadeea to settle by proclamation which " of the disputed computations should be regarded as the " true one."

To the best of Mr. Wilkins's knowledge, the Nadeea almanac is used all over Bengal, and the Benares all over the upper part of India : and it is likely, therefore, that the Tirhut is used all over Bahar ; but of the nature of this almanac I have no information ; only to judge from the date of the inscription found at Mongueer,† it is more likely to agree with the Nadeea than Benares patra.

As one of Mr. Wilkins's Benares patras came from Salsette, we may conclude that this almanac is in use in that part of

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* A district in North Bahar.

† Asiatic Researches, Vol. I. p. 127.

India. The inscriptions too, found at Salsette and Dehli,* confirm the opinions that this manner of dating is in use in both those places, as both are dated by the day of the bright side of the moon.

It appears from P. DU CHAMP, and P. PATOUILLET, and I believe I may add ABRAHAM ROGER, that in the part of India from which they write, the civil year begins at the new moon before the beginning of the astronomical year ;† which seems to shew that the Benares manner of dating is in use in great part of the coast of Coromandel ; but there is some reason to think, that in the neighbourhood of Madras and Pondicherry, they date in a manner different from that used either at Benares or Nadeea : for Mr. GENTIL makes the month Chitra or Sitterey, as he spells it, correspond with the sign Mesh, in which he agrees with an almanac published by an European at Madras, which seems to shew that in those places they date by solar months, but make Chitra correspond with the first sign.

Mr. WILKINS thinks he has heard of one or two places on the east coast of the Peninsula, and in particular Orissa, at which almanacs are computed ; but he is not acquainted with the nature of them.

I shall now give a more particular account of the three almanacs. The two Benares patras are preceded by a preface, which begins with an invocation to the Deity, and then gives a whimsical account of the four Yoogas, or ages, and of the inferiority of each succeeding age to that preceding it, and concludes with astrological remarks.

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* Asiatic Researches, Vol. I. p. 363, and 379.

† Narsapour, from which P. PATOUILLET writes, is near the coast, and in the latitude of $16°\frac{1}{2}$ N. Chrisnabouram, from which P. DU CHAMP's Memoir is sent, is in nearly the same latitude, but about 2° inland, and Paliacat, where ABRAHAM ROGER resided, is on the coast, in the latitude of $13°\frac{1}{2}$, or near $\frac{1}{2}$ a degree N. of Madras. This author, however, has expressed himself so inaccurately, that I am not sure whether they begin the year at that time or not.

There are no titles to any of the columns of which the almanacs are composed, nor is any explanation of them given in any part of the work; but by a careful examination of the numbers, a person acquainted with astronomical computations may, without much difficulty, find out their meaning.

The calendar part contains one page for each half of the lunar month. At the top of each page is given the year of the eras of Veekramādeetya and Sālavāhana. After this comes the name of the month, and in one almanac is given also the name and number of the month used by the Mahometans.

The part below this consists of eleven columns. The first gives the day of the month, according to the civil reckoning; the next the day of the week; and the two following contain the time of the day, that is the danda and pala at which the lunar teethee ends. The fifth column contains the name of the nakshatra * which the moon quits during the course of the day; and the two next shew the time at which she quits it.

The three next columns are very odd; they serve to shew the moon's place in what may be called a moveable zodiac, the first point of which moves backwards with the same velocity with which the sun moves forwards, and coincides with the sun at the beginning and middle of the Hindoo year. This zodiac is divided into twenty-seven equal parts, and the

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* Otherwise called the 27 lunar mansions.

first of these three columns gives the name of the 27th part which the moon quits during the course of the day, and the two others the time at which she quits it. I do not know what use these columns can be applied to, unless that of astrology. No trace of any thing of the kind has occurred to me in any account of the Hindoo astronomy *.

In these columns the names of the days of the week, and nakshatras, are expressed by the first syllable of the word.

The last column is the day of the month used by the Mahometans.

As no explanation of these columns is given in the almanacs, it will be proper to mention my reasons for supposing them to be such as I have asserted.

The numbers in the third and fourth column increase while the moon is near her apogee, and diminish during the rest of the month, which shews that it must be the time at which the moon completes some part of a revolution; and by examining these numbers during twelve revolutions of the moon in anomaly, it appears that the moon moves over 336 of these parts in 330^{d} $41^{dan.}$ $43^{pal.}$ which differs very little from the time answering to 336 teethees, so that there can be no doubt but that these columns shew the time at which the teethee ends. But a further proof of the truth of it is, that the time given in these columns for the end of the last teethee of each half month, agrees pretty nearly with the time of the new and full moon given in the nautical almanac, after allowing for the difference of longitude between Greenwich and

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* From a circumstance not worth mentioning, I find that the place of the moon in this moveable zodiac, is called the Yug.

Benares, and the time between sun-rise, at the latter place, and noon; which shews also that the time in these columns is reckoned from sun-rise, as might naturally be expected.

In regard to the moon's place in the nakshatras and moveable zodiac, it appears, by examining the fifth and eighth columns, that in each of them are 27 characters, which return constantly in order, except when the regularity is broken, either by the moon quitting two spaces in the same day, or by not quitting any one space in the day. The numbers also, both in the sixth and seventh, and in the ninth and tenth columns, increase when the moon is near the apogee, and diminish when she is near the perigee, which shews that they must be the time at which the moon finishes some 27th part of a revolution of one kind or other; and by examining the alteration of the numbers during twelve revolutions of the moon in anomaly, it appears first, that the moon describes 326 of the spaces given in the fifth column, in 329^{d} $57^{dan.}$ $38^{pal.}$ which is the time in which the moon moves over that number of nakshatras; and secondly, that the moon describes 350 of the spaces given in the eighth column in 329^{d} $36^{dan.}$ $48^{pal.}$ which is the time in which the sum of the mean motions of the moon and sun are equal to 350 27ths of a circle; or in other words, is the time in which the moon's motion in the moveable zodiac is 350 of these 27th parts; and moreover, I cannot find any other 27th of a revolution of the moon which will agree with this time; which is a sufficient proof that the numbers in the ninth and tenth columns are the times at which the moon quits one of these 27th parts in the moveable zodiac. But a thing which more strongly proves the truth of this, and which also shews that the first point of this moveable zodiac coincides with the first point of the fixed zodiac, when the sun also coincides with it, is this: according to my supposition it is evident, that whenever the sun quits a nakshatra at the same time that the moon quits some other nakshatra, the moon must at the same time quit some 27th part of the moveable zodiac; and consequently that the numbers in the ninth and tenth columns should agree with those in the sixth and seventh; and accordingly we find, that on all the days of the year, in which the sun quits a nakshatra, the numbers in these two pairs of columns are nearly alike.

Underneath these eleven columns are tables of the diurnal motion and places of the sun and five planets, and of the moon's node in the Hindoo zodiac, for each week of the year; and between these tables and the eleven columns is set down the day of the month and week, and number of the week for which these places are given, and also the interval at that time between sun-rise and midnight, and the length of the day. The day of the week for which these places are given, is that which is the first in the current solar year, and the number of the week is also counted from the beginning of the solar year. The places are given for midnight.

On the right hand of the eleven principal columns is a space allotted for miscellaneous occurrences. In this is set down the time at which the sun enters each sign, and the beginning and end of eclipses. In these two years no solar eclipses were visible, but the end of the lunar eclipse is denoted by a Sanskreet word, signifying delivery; the meaning of the term used for the beginning is not so clear. The number of digits eclipsed is not set down. The other articles in this space consist chiefly of the time at which the moon and planets come to certain situations. Of this there is not a great deal which I understand, and what I do, is not worth taking notice of. There are also some figures and tables between the preface and calendar, which, as far as I can find, relate only to astrology.

The Nadeea almanac contains, besides the articles abovementioned, the time of the day at which the lunar teethee ends, the number of the nakshatra and yug (place in the moveable zodiac) which the moons quits on that day, and the time at which she quits them, besides a few occasional remarks. It is disposed in a much coarser manner than the Benares patra, as each page contains as many days as it will hold, so that the month seldom begins at the beginning of a page. It contains no preface, and no explanation of the columns. The days of the week are not denoted by the first syllables of the name, but only by a number, expressing their order in the week, which caused some trouble in finding what day was meant by these numbers; but, by a variety of circumstances, I think it certain that the number 1 must denote Sunday.

XXI. *On Evaporation.* *By* John Andrew de Luc, *Esq. F.R.S.*

Read June 28, 1792.

In my last papers on hygrometry, I have considered moisture in the air as a modification of a particular fluid, produced by the evaporation of water, composed of water and fire, mixed with the air, but independent of it. However there was a more common theory of that phænomenon, in which evaporation was attributed to a dissolution of water by air: but as an inquiry into the cause of evaporation belongs more to hygrology than to hygrometry, I made then no remark on that subject; having in view some experiments which were to ascertain a particular point fundamental to it. Since that time I have made those experiments, which are the object of this paper; but before I relate them, it is necessary to explain how they connect hygrometry with hygrology; which will be by stating the principles of those two branches of experimental philosophy according to my system.

From the time I fixed my attention on evaporation, and its various consequences, I was led to think, that the kind of dissolution of water, observed in those phænomena, was operated by fire, without any interference of air: and among other reasons for that opinion, the most decisive for me was, that every liquid cools when it evaporates; for I considered that circumstance as a proof, that the portion of the liquid which then disappears, is carried away by a quantity of fire proceeding from the liquid itself.

The general phænomena of evaporation and moisture had been my only guides, when I first published my opinion in that respect in my work *Rech. sur les Mod. de l'Atmosphère*: but it was then in a very imperfect state, and I owe much to Mr. Watt, the inventor of the new steam engine, for the degree of precision which that theory has acquired in my last work, *Idées sur la Meteorologie.* In particular, I am indebted to that truly ingenious and learned experimental philosopher, for an immediate proof of my fundamental opinion, resulting from an experiment, that he was so good as to repeat in my presence, and which demonstrates, that in the common evaporation of water in open air, the quantity of heat lost by the mass, bears to the quantity of water carried away, a proportion still greater than that which is found in the steam produced by boiling water. Therefore there is no reason to doubt, that steam is formed in the first, as in the last of those cases.

It is true, that in some other respects, appearances are very different in those two classes of phænomena; but those differences proceed from well determined causes, as will appear by the following abstract of my theory.

On the Laws of Hygrology.

1. Whenever water is in a state of evaporation, an expansible fluid, composed of water and fire, is produced. To that

fluid, the object of hygrology and hygrometry, I shall give the name of steam, in every case.

2. As long as steam exists, it has a power of pressure, as air itself ; but it does not belong to the class of permanent fluids, for it may be decomposed by a certain degree of pressure, or cooling, according to determined laws.

3. The characteristic property of steam, from which proceeds its decomposition by those causes, is, the having a fixed maximum of density by a given temperature, which increases with the temperature. Thus, when that fluid is arrived at the maximum correspondent to a certain temperature, it must undergo a decomposition, either by cooling, because its actual maximum is too great for the new temperature ; or by an increase of pressure (the temperature remaining the same), because its density becomes too great for that temperature.

4. The degree of pressure exercised by steam, or which it can support without decomposition, being, *cæteris paribus*, proportional to its density, depends, in the same manner, on temperature.

5. Steam is formed by every temperature, when a previous space permits its expansion ; but, according to the above law, no steam can be formed when, for its formation, it ought to repel an obstacle superior, in the smallest degree, to the maximum of its power by the actual temperature ; and if it has been formed under a pressure not exceeding its power, if that pressure increases, or the temperature lowers, ever so little, it is totally decomposed.

6. This is what determines both the degree of heat by which water can begin to boil, and the variations of that degree, by those of pressure. For ebullition is that state of

a liquid in which steam is continually formed within itself, notwithstanding the external pressure; and as such an expansive power in steam depends on a certain degree of density, its production requires, in the liquid, a certain degree of heat, which thus is determined by the degree of pressure. As for the fixity of the degree of heat in water boiling under a constant pressure, it proceeds from an equilibrium constantly produced between the quantity of fire which continues to penetrate the water, and that which goes off in the steam; the differences which happen in the first of those quantities, having no other sensible effect, than to produce a more or less rapid formation of steam.

7. Now from the same law derives also the difference between the phænomena of common evaporation and ebullition. If the latter requires a determined degree of heat, it is only because the steam cannot be formed within the water, without having at once a sufficient degree of density for overcoming alone the actual pressure exercised over that water by the atmosphere. But in common evaporation the steam is formed at the surface of the water by every temperature, because there it meets with no resistance but what it can always overcome: for it only mixes with the air, and expands it in proportion to its quantity, as would a new quantity of air.

8. Steam formed by common evaporation is absolutely of the same nature with that of boiling water; and in respect of the pressure that it undergoes, it is in the same state as when produced by evaporation under an exhausted receiver. In this case, where the pressure of the atmosphere is suppressed, the resistance which steam meets with in the space is its

own, and consequently it is proportional to its own power; and in open air, the part of the whole pressure incumbent on steam is, to that whole, as its power is to that of the whole mass, the rest of the pressure being supported by the air with which it is mixed: which proportion in the pressure that steam undergoes, in this case, comes exactly to that of the first. This is agreeable to the laws of expansible fluids, and the following example, taken at a determined temperature, will prove it from experience.

9. The thermometer being at about 65° of FAHRENHEIT, the maximum of evaporation, in an exhausted receiver, keeps a column of quicksilver of 0,5 inch suspended in the short manometer (I have quoted the experiments which give that middle result). That phænomenon has been considered by some natural philosophers as distinct from common evaporation, and depending on a disposition of the particles of liquids of repelling each other; which disposition, supposed to be counteracted by the pressure of the atmosphere, must appear when that pressure is removed. But, besides the known loss of heat by the liquid, in this case as well as in open air, which points out the same cause of evaporation, an equal pressure is also produced, and added to that of air, when the receiver is filled with the latter; which again indicates the same effect, as will appear in the following example.

10. If, by the above temperature, the receiver is filled with air of the same density as the air of the place; in which case a barometer inclosed in that receiver will stand at the same height as in the open air; and that in the receiver, being then very dry, water be introduced in a quantity sufficient

for producing in it the maximum of evaporation, the inclosed barometer, considered here as a manometer, will rise 0,5 inch; as Mr. DE SAUSSURE has found it by experiments.

11. If now we consider that the instrument called barometer, is in every case a manometer, we shall find, that those phænomena observed in close vessels, give us a true idea of what happens to steam in the atmosphere. When that instrument is observed in open air, it is with reason called barometer; since the expansive power exercised on its mercurial column is determined by the weight of the incumbent column of air; therefore it is the true measure of the weight of that column. But at the same time, and more immediately, it is a real manometer, since the immediate action exercised on its column, and to which this is also proportional, is, *cæteris paribus*, exercised by the density of the fluid, or fluids, which surround it, whose action is exactly the same as if any part of the mass of those fluids were suddenly secluded from the rest in a close vessel. Therefore, when steam is mixed with air, be the mass shut up in a vessel, or be it a certain part of the atmosphere distinct by its place, both fluids will act on the column of the manometer or barometer, or on every obstacle, and thus against each other, according to their respective power; for this fundamental reason, proceeding from the nature of steam as now explained, "That no mechanical cause can produce the decomposition of that fluid, "but by forcing its particles to come nearer each other than "the actual temperature can permit:" which case cannot happen in the atmosphere, except by the accumulation of steam itself in some part of it; since, elsewhere, it only remains mixed

with the air according to its own laws, as if there were no air.

The whole theory of hygrology, as far as I have been able to collect the phænomena of that class, appears to me comprehended in the foregoing propositions, founded on facts. The objects of that science are in general the cause of evaporation, and the modifications of the evaporated water. The common source of the water thus disseminated in the atmosphere, is the surface of the earth; whence, in spontaneous evaporation, both in air and *in vacuo*, as well as in ebullition, we see that water fly off with latent fire. If we collect that product in a close space, it acts in the same manner as a new quantity of expansive fluid. We know from experience, that an expansive fluid is really produced by ebullition, and by evaporation in an exhausted vessel: there is no reason why the cause of evaporation, and its product, should change in any case, only by the presence of air; and in examining what may happen in open air, we find no particular cause of the destruction of that expansible fluid, nor any difficulty in conceiving its dissemination in every part of the atmosphere.

But here we lose sight of steam, for it is as transparent as air itself: here also its mechanical action is as little perceivable as that of any set of scattered particles of air; and though its specific gravity is much less than that of air, its quantity existing in the atmosphere is most times so inconsiderable, that it can hardly be discovered by that means, on account of other causes which also affect the specific gravity of a given mass of free air. Therefore, notwithstanding our experiments on the formation of steam and its effects in our vessels, we should be ignorant of its functions in the atmosphere, if it were not for its property of producing moisture, by which we may trace it wherever it is, and determine its quantity. Here then a new field is open for experiments and observations; since by connecting hygrometry with hygrology, the hygrometer is for us in the atmosphere, what the manometer is in close vessels.

The particular experiments which I have to relate, have that connection in view; as they will shew, that in a close vessel, either filled with air, or free from it, the product of evaporation affects, at the same time, the hygrometer and the manometer; the former by moisture, the latter by pression. But I must here take this as a fact, in order not to interrupt the course of the theory, which, from hygrology, leads to hygrometry, by certain laws demonstrated in the fluid which is the cause of moisture.

Of the Laws of Hygrometry.

1. The science of hygrometry derives its origin from the cause whence proceeds, that the density of steam has different maxima, according to the temperature. I have shewn, in my last work on meteorology, that in the particular association of water and fire which produces steam, both of those ingredients retain the faculty of producing their respective equilibrium between the medium and bodies; that also the particles of water retain a tendency of uniting together, which begins to be effectual at a certain distance: and that their union takes place when they come to a degree of proximity, by which

they can surmount the cause which keeps them disseminated in the space, namely, fire. The less the quantity of free fire (or of the cause of heat) in a space, the greater is the distance at which the particles of water have the faculty of uniting together, and also of following other tendencies, by abandoning their latent fire; and *vice versa.* A final union of a sensible quantity of particles of water in the medium (or, what comes to the same, that precipitation of water which first appears in it as a mist) takes place, when the density of steam is arrived beyond its limits; and those limits depend on temperature, because, the greater the quantity of free fire in the space, the nearer the particles of steam may come to one another, without a final union of their water. The whole *in vacuo*, as in air. Such is the physical principle common to hygrology and hygrometry.

2. Hygroscopic substances are of three distinct classes. Some seize on the water of steam, by a chemical affinity with that liquid; among these are acids, salts, and calces. Some only imbibe it by its tendency to propagate itself in capillary pores; but, from their nature they receive no sensible increase in their bulk by its introduction; in the number of these are porous stones. Lastly, some substances, which also only imbibe a certain quantity of water, are thereby expanded; and these are most of the solids belonging to the vegetable and animal kingdoms. Various hygroscopic phænomena, which only depend on the different properties of the substances themselves, being thus foreign to the fundamental laws of hygrometry, I shall here confine myself to the last class, which appears the only proper one for that general purpose; and, among the hygroscopes of that class, I shall only consider

those which cease to lengthen, only when they cannot be penetrated with more water; as, by the use of these, there cannot be any fallacy in respect of the most important phænomena to be determined, namely, the more or less intensity, according to certain circumstances, in the cause which affects the hygroscope placed in a medium.

3. The word moisture, like some others which also have been applied to certain phænomena the causes of which were not determined, is ambiguous in common language, implying sometimes the idea of a cause, and at other times that of an effect, without a proper determination; that defect might be corrected by means of different words for the cause and for the effect: but neologisms are often so troublesome, and sometimes so arbitrarily introduced into languages, that I shall only endeavour to determine the sense of the word moisture, according to cases, so as to avoid ambiguity.

4. Moisture, taken in a general sense, may be considered simply as invisible water, producing observable phænomena. Thus, in hygroscopic bodies, the quantity of water which expands them, and increases their weight, is concealed within their pores; and in the ambient medium, that water which affects hygroscopic bodies, being there under the form of steam, is as invisible as air itself.

5. But in respect of hygrometry, where moisture is considered as having correspondent degrees in the medium, and in hygroscopic substances, that word requires a more particular determination, on account of those two different situations of invisible water. Moisture may be either totally absent, or absolutely extreme, both in hygroscopic bodies, and in the ambient medium; which circumstance, on both sides, affords

a fixed module for determining correspondent degrees; but these modules are not of the same nature; and thence, in their relation to one another, both in the whole and in correspondent parts, moisture assumes in the medium, the character of a cause, and in hygroscopic bodies, that of an effect, as will appear by the following determination of both cases.

6. Moisture is totally absent, first in the medium, when it contains no steam; then (as a consequence) in hygroscopic bodies, because they contain no more water than can evaporate, without a decomposition of their component parts. The case supposed in that definition, is when, by some adequate cause, no sensible quantity of steam is permitted to remain in the medium; as in my lime-vessel. Moisture is extreme, first in the medium, either air or a space free from air, when no more steam could be introduced into it, without a part of it being decomposed; and then (also as a consequence to be explained) it is extreme in hygroscopic bodies, because no more water can be admitted into their pores. Now, from the nature of the last of those maxima, the quantity of water which produces it in a given body is fixed, since it is determined by the actual capacity of its pores; whereas the quantity of water which produces extreme moisture, in a medium of a given extent, is as variable as the temperature: therefore, the hygroscopic equilibrium between the medium and hygroscopic bodies in different stages of moisture, which equilibrium is the object of hygrometry as a science, does not depend on certain quantities of water contained in the first, of which the last may receive their share; it depends on different aptitudes of the steam contained in the medium to communicate water to those bodies; which aptitudes vary, not

only by the different densities of that fluid, but in steam of the same density according to the temperature.

7. In order to assign the cause of that equilibrium, we have previously to determine, 1. Whence proceeds a maximum of length in the hygroscopic bodies here referred to; 2. How that maximum is produced by steam; 3. Why it takes place by different densities of that fluid, when the temperature is different.

8. Water introduces itself into the above bodies by its property of adhering and propagating itself on their surface, and thereby of entering their capillary channels; and those bodies resist its introduction, which expands them, by the tendency of their particles to remain united. This last tendency, from its own law, being less and less in proportion as the particles of the body are already less in contact with each other, it should appear, that water ought at last to divide them entirely: but, according also to the law of that propagation of water, the greater is its quantity already induced on the internal channels of the body, the less is the tendency of new particles to enter them; and this last diminution being more rapid than the former, there comes a point in which the two forces are in equilibrium; which circumstance determines the maximum of the introduction of water, and consequently that of the lengthening of the body.

9. The maximum of length of those hygroscopic bodies is thus produced, whenever a sufficient quantity of water stands or floats round them, without having a stronger tendency elsewhere. Therefore that maximum is produced when such a body is immersed into water; and it is also produced when the same substance stands in a space where

steam is arrived at its maximum; for that fluid is then so ready to abandon its water, if this has a tendency elsewhere, that a small quantity of new steam introduced in the space, would occasion the decomposition of part of it, followed by a spontaneous precipitation of water.

10. But we have seen above, that steam arrives to its maximum with different degrees of density, according to the temperature; consequently, whatever be the actual density of steam, if that density is its maximum conformable to the temperature, the hygroscopic body will receive water to the maximum; and thereby it will attain its maximum of length.

11. Now, as according to the premises, the density of steam is not the only circumstance by which, at the last period, the hygroscopic body retains in a medium the maximum of water that it can receive, that effect depending also on the temperature; by the same cause, the density of steam is not at the former periods, the only circumstance which determines the quantity of water that the hygroscopic body can retain in the medium; that quantity depends also on the temperature. The following is the theory of that phænomenon, or of the hygroscopic equilibrium between the medium and bodies.

12. In consequence of the above determined composition of steam, whatever be its quantity in a given space, either filled with air or free from it, its aptitude of communicating water to hygroscopic substances, is always proportional to the actual ratio between that quantity and the maximum correspondent to the temperature; which ratio, and not an absolute quantity of steam, is a certain degree of moisture considered in the medium; whereas moisture considered in a

certain hygroscopic body, is the determined quantity of water which it can retain in the medium, according to the actual *ratio* in the steam. Moisture in the medium is greater, and an hygroscope retains in it a greater quantity of water, when the actual quantity of steam bears a greater proportion with its maximum correspondent to the temperature; and *vice versa*. Therefore the divisions of the instrument are meant to indicate, by every temperature, the actual distance of moisture in the medium, either from its zero, or from its maximum; and that distance is the degree of moisture. But this is yet without any reference to the density of steam; the determination of this having the temperature for one of its indispensable elements.

13. From that derives the remarkable phænomenon of the effects produced on moisture in the medium, by the variations of temperature, though the quantity of steam, or evaporated water, remains the same in the same space; effects of which we are informed by the variations of the hygroscope, and which are the same by the absence or presence of air. Every change in the temperature, operates a correspondent change in the module of moisture; or in a variable *totum*, of which, according to its magnitude, the actual quantity of steam is a greater or a smaller aliquot part; that module being always the quantity of steam that could not be exceeded in the space, by the actual temperature, without the decomposition of part of it; which is always extreme moisture in the medium.

14. From that results lastly; that if, besides being acquainted, by the hygroscope, of the degree of moisture in the medium, or of the *ratio* between the density of steam and its actual module, it is required to know the density of that

steam, or its quantity in a given space; the observation of the temperature is required: as this, by previous experiments in order to determine the quantities of steam correspondent to its different maxima according to the temperature, will afford a co-efficient for the *ratio* observed.

15. But are we permitted to consider the variations of the hygroscope as proportional to those of moisture in the medium? This, according to the above determinations, would be the case, if the hygroscopic substance of the instrument lengthened in proportion to the quantity of water that it may retain in the medium. But the cause of the expansion of those substances by water, and the capacity of their pores at different periods of moisture, are too complicated for answering that question *à priori*; and by experience, the great differences observed in the marches of many of those instruments made of different substances, prevents us from assigning that property to any of them, till some particular experiment comes to help us in that respect. However, that circumstance affects only the practical part of hygrometry, and is foreign to the fundamental principles of that science, which I have now exposed as they appear to me from its general phænomena.

I have indicated, in my last paper, two means which I had formerly imagined for obtaining that desirable and still wanting correspondence between the march of a determined hygroscope, and that of moisture in the medium. One of those means was, to observe, at the same time, the variations in weight and length of the same substance, in order to compare the quantities of water which it retains, with their effects on its length. I have executed that experiment; but its results, which I gave in my last paper, have confirmed my doubts, on even the acquisitions of weight being proportional to the increase of moisture in the medium ; since they do not keep the same pace in different substances.

The other means was, to introduce in a dry vessel successive equal quantities of water without opening the vessel, and to observe their effect on the hygroscope. I made, last year, a first attempt of that experiment, which succeeded in respect of the introduction of water in a space of a known small degree of moisture ; but the event confirmed also the uncertainty that I suspected in that method, because of a variable share of water retained by the vessel itself. However, as some parts of my apparatus might contribute to that uncertainty, I intend to correct it, and repeat the experiment ; which, if I obtain regular results, I shall have the honour to communicate to the Royal Society.

I have now summed up the series of propositions which connect together in one system the whole of the fundamental phænomena of hygrology and hygrometry ; and the only part of that system which remained to be proved by immediate experiments is, that *link* mentioned above between the two classes of phænomena, namely ; " That *in vacuo*, as in air, " the product of evaporation affects the hygroscope as it " does the manometer." I had ascertained that fact since the year 1776, by immediate experiments ; which was the reason why, in my last work, I considered that circumstance as a principle, which, at least, could not be denied by any thing known to the contrary. But, for an immediate demonstration of it, many conditions were wanting in my first experiment, which could not be obtained but by the whole of my subsequent labour. That experiment is now made with a sufficient degree of regularity; and the more so, as it has been executed by Mr. HAAS, in one of his air-pumps, with some of my whale-bone hygrometers, made by himself: and I shall now give its result.

Experiments on Evaporation, in Air and in Vacuo.

The purpose of these experiments being to shew, that the effects produced by evaporation on the hygrometer and the manometer, are the same in the absence of air, as when they take place in air; I shall begin here by referring to Mr. NAIRNE's paper, in the Philosophical Transactions for 1777, in which it is proved by experiments, that Mr. SMEATON's pear-gage is not a true manometer, since it does not indicate the presence of steam, when some remains in an exhausted receiver; but that it is the only true measure of the quantity of air itself remaining in that space.

That property of the pear-gage, proceeds from the fifth law of the above theory of hygrology. When, in the common temperature of the atmosphere, steam is unprotected by a sufficient mixture of air, it is totally decomposed by the pressure of the atmosphere, when this acts by the interposition of some sort of piston, as is the column of quicksilver in the pear-gage: therefore no fluid can remain at the top of that instrument when filled with quicksilver, except the quantity of air which was contained in its capacity before the introduction of the quicksilver.

When some lasting cause of formation of steam happens to be within the air-pump, and that the pump is long worked, the air is at last almost completely exhausted, and the remaining pressure acting on the manometer is that of steam. Mr. NAIRNE has shewn me an instance of that effect, in which the quantity of air indicated by the pear-gage, was not above $\frac{1}{10000}$ part of the whole. That remarkable fact will prove, that in the following experiments, air itself was exhausted to the utmost degree, and thus reduced to nought in respect of any influence on the main phænomena.

I must also premise, whence proceed some anomalies which will be observed in those experiments. It is in general from the nature of steam, from which, in our experiments, it is submitted to certain modifications, depending on its laws, but which cannot be always referred to them by immediate explanation, because of undetermined effects of the sides of the vessels, and of irregularities in the temperature.

The law of steam, which is the most subject to anomalies by those causes, is that of two distinct maxima, one of production, the other of preservation. Those two maxima coincide, when the temperature is near the freezing point; as in that case, the whole quantity of steam that can exist at one time in a space, is also produced when there is a sufficient quantity of water in any part of it; whereby the hygrometer stands at 100, which is its point of extreme moisture. But in proportion as the whole apparatus grows warmer, the maximum of production falls short of that of preservation; the quantity of steam does not increase, though it might be more dense without undergoing a decomposition; and thereby the hygrometer recedes from extreme moisture.

Such is the general law of steam, as it results clearly from the whole of my experiments; but in particular cases, it is subject to anomalies from various causes, among which are the following. If the water that evaporates is warmer than the space which receives the steam, more moisture is produced in that space, or the quantity of steam is greater in it than by an equal temperature in both; and *vice versa*. More or less distance of the part of the space where the hygrometer stands from the sides of the vessel, produces also anomalies; as according to their own state of moisture, if near enough, they have an influence on moisture in that space, as I have explained in my former paper; and this is often the case in some measure when the vessels are too small. Lastly, differences in the temperature of the whole or of some part of the vessel, comparatively with that of the space, are the most common causes of anomalies; for steam is alternately decomposed and reproduced by those differences, and when they have once begun in a vessel, there is no certain means to bring it to a regular course of phænomena, except by beginning again, or by a long equal temperature.

I come now to the experiments, in which I shall indicate some effects of those causes.

FIRST EXPERIMENT.

The receiver used was about eight inches in diameter, and twelve in height. One of my hygrometers was suspended in it, with a thermometer, the degrees of which were actually divided in ten parts. In the evening, a very large wet cloth was placed in loose folds on its bottom, and the first observation was made the next morning as follows.

Time.	Long Gage.	Short Gage.	Hygrom.	Therm.
7^h 5′ A. M.	0.0	——	96,5	47,75

The maximum of evaporation was then surely produced; but it remained short 3,5 degrees from extreme moisture: this was in air.

I could not have any indication of the quantity of pressure produced by the steam, mixed then with the air; for a receiver, equally pressed in and out, is not air tight. An experiment for determining that increase of pressure, must be made in a sealed vessel, as in Mr. DE SAUSSURE's experiment, by which he found that important phænomenon: but from that experiment we are sure, that if a barometer had been inclosed in the above receiver, being then sealed, the steam produced would have made it rise, at the same time that it affected the hygrometer.

The pump was then worked; after which the instruments were found as follows.

7,15 26,0 ——— 97,0 44,75

I shall make here a remark on the known phænomenon of the diminution of heat, when a space filled with air is suddenly exhausted; as in this case we have an analogy that will enhance the most natural explanation of that phænomenon. Free fire, the cause of heat, being an expansible fluid, as air and steam, is then carried away with them, notwithstanding its tenuity, and heat is diminished in the space till new fire is come through the vessel. Now, the same is observed in respect of moisture with a quick hygrometer, for it goes for an instant to dryness, while the quantity of steam carried away is repairing by a new evaporation.

In the above case, moisture was lastly increased 0,5 degree; yet the quantity of steam was less than before, and the cause of that increase of moisture was, the diminution of its module by that of heat.

In respect of air, its quantity was so diminished, as to permit a column of quicksilver of 26 inches to rise in the long gage.

The pump was again worked, and time given for the instruments to settle.

7,25′	28,7	——	96,5	47,75

The original temperature was now restored; and as air had been much diminished by the first operation, fire, thus much more free in this, escaped the action of the pump. Moisture also was restored nearly to its original degree, though very little air remained in the receiver.

The pump was worked by intervals.

10,5′	28,5	0,70	95,66	47,75

The quantity of air was now so little, that it cannot be supposed to have had any share in the effect of the steam; however moisture remained sensibly the same by the same temperature.

Worked again the pump to the best vacuum.

10,10	28,7	0,53	94,5	48,8

The pressure on the short gage was now, according to Mr. Nairne's experiments, almost wholly produced by steam; yet moisture preserved its own distinct modifications: here it is diminished, notwithstanding the maximum of evaporation, because of the increase of heat.

The pump was worked at different times, for extracting some air that was disengaging itself from the large wet cloth.

11,35′	28,7	0,53	91,0	51
2,50 P. M.	28,72	0,60	86,75	54,15

Here again, notwithstanding a constant maximum of evaporation, moisture continued to diminish by the increase of heat, as it does in air.

The apparatus was left in that situation about four hours; and the observers having left the room, this, and the approaching evening, occasioned a sensible diminution in the heat; whereby, as the sides of the vessel cooled before the inclosed space, the steam began to be decomposed against them, and they were found covered with dew and streaming water, by a real continuing distillation. The last cause of anomaly above mentioned is here conspicuous by its great effect; for, during that distillation, and consequently though new quantities of steam ascended continually through the space, moisture was in it much less than in the former observations, as will be seen by the following, made before and after working the pump. The thermometer could not be observed, on account of the thickness of the dew on its side of the vessel.

4,20′	27,35	———	76,0	———

Worked the pump.

4,35′	27,75	0,62	76,0	———

The steam continued some time to decompose itself against the sides of the vessel, and moisture to remain in that state of diminution, till the heat communicated by the observers to the apparatus made the internal dew evaporate; and when thereby the thermometer could be observed, the instruments were as follows.

6,47′	28,18	0,6	96,0	52,75

Here also is an instance of what I have mentioned above, of the irregularities attending all the phænomena, after some water has been once deposited on the sides of the vessel: moisture was too great for the temperature, and it remained in that state the whole evening.

7,55′	28,82	0,6	96,5	52,75
8,4	28,82	0,6	96,5	52,—

But the next morning, after the dissipation of some dew that was on the inside of the vessel, and after having worked the pump, the instruments were found as follows.

7,35′ A. M.	29,12	0,6	97,66	44,75

Now this, in respect of moisture, and its correspondence with the temperature by the maximum of evaporation, is sensibly the same, as when the receiver was filled with air at the beginning of the experiment, though at this time no sensible quantity of air remained in it.

SECOND EXPERIMENT.

Every part of the apparatus was the same in this experiment as in the former, except the receiver, which was only about six inches in diameter, and eight in height; to which circumstance may be assigned, that moisture was generally greater comparatively with the temperature; but in respect of the indifference of air to the phænomena of moisture, the results were the same.

A wet cloth, only sufficient to keep up the maximum of evaporation throughout the whole experiment, was used this time; by which means no air appeared in the receiver after the disengagement of the little quantity contained in the small cloth. This was put under the receiver, filled with air, at about four o'clock in the afternoon: and the first observation, made in that state, was thus:

8,— P. M.	0,0	———	95,0	65,75

The pump was then worked, for extracting a part of the air, and time given for the instruments to settle, and they were found,

8,15′	6,5	———	94,0	65,75

After this observation, and after each of the following, the pump was worked, and time also given for the instruments to settle.

8,30′	13,0	———	91,0	66,75
8,45	19,0	———	86,5	67
9,15	25,0	———	86,0	67,75
9,30	29,3	0,65	85,0	67

After this, the pump was no more worked the same day, but the two following observations were still made.

10,0′	29,23	0,65	89,0	66,5
11,0	29,23	0,65	95,0	64,5

The apparatus was left in that situation till the next morning, when the following observation was made before working the pump.

6,20′ A. M.	29,35	0,55	97,5	63,15

The pump was then worked, and the best possible vacuum was produced, in which, according to Mr. Nairne's experiments, there could not be any sensible quantity of air.

6,40′	29,40	0,50	96,5	63,15

In comparing the results of this experiment with those of the first, moisture, as I have said, is generally greater in proportion to the temperature. But, setting this aside, and comparing the motions of the hygrometer and the thermometer, it is evident, that they are independent of the modifications of air ; and that it may safely be concluded : " That " the product of evaporation is always of the same nature, " namely, an expansible fluid, which, either alone or mixed " with air, affects the manometer by pressure and the hy- " grometer by moisture, without any difference arising from " the presence or absence of air ; at least without any per- " ceived hitherto."

XXII. *Supplementary Report on the best Method of proportioning the Excise upon Spirituous Liquors. By* Charles Blagden, *M.D. S.R.S.*

Read June 28, 1792.

THE Report to which this paper is intended as a Supplement, was drawn up, and published, when our experiments on the specific gravities of the spirituous liquors had been continued only to equal quantities of alcohol and water by weight. It was foreseen that a further set of experiments, on more dilute liquors, would be wanted: but as these must necessarily take up a considerable time, the persons concerned thought it best to submit those already made to the public; that if any errors or inaccuracies should be discovered they might be avoided, and if any person should suggest a better method it might be adopted, in the subsequent proceedings. Want of ice, and some other hinderances, prevented the experiments on what may be called watery mixtures from being entered upon earlier than the beginning of last winter. Fresh spirit was distilled for the purpose by Mr. SCHMEISSER, who brought some of it to the specific gravity of ,817; but it had a smell somewhat different from that employed in the former experiments, and more approaching to the odour of ether. On inquiry we found, that, whereas Dr. DOLLFUSS had drawn the former spirit off vegetable alkali, Mr. SCHMEISSER used GLAUBER's salt calcined by exposure to the air. In order to try whether this circumstance made any difference in the quality of the new spirit, Mr. GILPIN mixed some of it with an equal weight of water, and afterwards brought the mixture to all the different temperatures from 30° to 100°, operating in the same manner as he had done with Dr. DOLLFUSS's spirit; when the specific gravities were found to come out the same. Mr. SCHMEISSER's alcohol, therefore, was used without hesitation. As no censure had yet been passed on our former experiments, the same general method was pursued for the new series; with a small variation, however, the reason of which is now to be explained.

In the Report on the first experiments I introduced the following remark. "It must be observed, that Mr. GILPIN "used the same mixture throughout all the different tem"peratures, heating it up from 30° to 100°; hence some "small error in its strength may have been occasioned, in the "higher degrees, by more spirit evaporating than water; but "this, it is believed, must have been trifling, and greater in"convenience would probably have resulted from interposing "a fresh mixture." The consciousness that such a source of error existed, made us desirous of ascertaining to what quantity it amounted, by some previous experiments, before the new set should be begun. These shewed that it was somewhat greater than had been supposed, though not such as ever to cause a difference of more than a single unit in the third place of decimals, even in the temperature of 100°. The greatest difference found, in that degree of heat, was ,00094; and in a heat of 80°, the highest to which the tables for use were to be carried, it amounted only to ,00064; being in both cases greatest when the mixture consisted of 85 parts of water, by weight, to 100 of alcohol. This difference, however, small as it was, afforded sufficient reason for repeating all the former experiments, conjointly with the new set for dilute spirits, so as to make one entire series, with the same spirit, and executed throughout in an uniform manner. To obviate the error from evaporation in this series, and ascertain what each mixture really lost of its strength during the operation, all the fluids were first weighed at 60°, before they were cooled down to 30°; from 30° to 100°, they were weighed at every 5 degrees, as before, consequently a second time at 60°; and lastly, after having been heated to 100°, they were again brought to 60°, and weighed at that point a third time. The difference between these weights, at the beginning, middle, and end of the experiment, was applied, in due proportion, to correct the numbers of the respective intervals between them; by which means it is believed that the error arising from the gradual evaporation of the spirit, during the experiment, has been made to disappear. Mr. GILPIN having also observed, that the spirits adhering to the sides of the funnel which he employed to fill up the weighing-bottle, grew weak by the evaporation, and so diluted the fresh spirit poured into the funnel, determined to use a smaller instrument of this kind, namely, such an one as would not hold more than 15 grains of spirit; in which a less surface being left wet when the spirit ran out, the error from this cause would be proportionably diminished.

Under all these precautions were the experiments made, of which the results are given in the following tables. They are drawn up exactly like the tables in the former Report;

but, as alcohol was taken for the fixed quantity in the first half, so water is taken for the fixed quantity in the last half, which, therefore, consists of mixtures containing all 100 grains of water, with 95 grains, 90, 85, and so on successively, of alcohol, till the last column is pure water. This arrangement will be clear to every one, on reading the title of each column of the tables. The first table gives the actual weights, at the even degree of the thermometer, corrected for the evaporation; and the second table gives the specific gravities, calculated from those weights, with the same allowances and corrections as were specified in the original Report.

TABLE I.

Weights at the different degrees of temperature.

Heat.	The pure spirit.	100 grains of spirit to 5 grains of water.	100 grains of spirit to 10 grains of water.	100 grains of spirit to 15 grains of water.	100 grains of spirit to 20 grains of water.	100 grains of spirit to 25 grains of water.	100 grains of spirit to 30 grains of water.	100 grains of spirit to 35 grains of water.	100 grains of spirit to 40 grains of water.	100 grains of spirit to 45 grains of water.	100 grains of spirit to 50 grains of water.	100 grains of spirit to 55 grains of water.	100 grains of spirit to 60 grains of water.	100 grains of spirit to 65 grains of water.
°	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.
30	2487,35	2519,92	2548,42	2573,80	2596,66	2617,30	2636,23	2653,73	2669,83	2684,74	2698,51	2711,14	2722,89	2733,87
35	2480,87	2513,43	2541,84	2567,26	2590,16	2610,87	2629,92	2647,47	2663,64	2678,60	2692,43	2705,14	2716,92	2727,87
40	2474,30	2506,75	2535,41	2560,74	2583,66	2604,50	2623,56	2641,08	2657,23	2672,30	2686,32	2698,94	2710,81	2721,83
45	2467,62	2500,14	2528,75	2554,09	2577,10	2597,98	2617,03	2634,64	2650,87	2666,04	2679,99	2692,77	2704,57	2715,62
50	2460,75	2493,33	2521,96	2547,47	2570,42	2591,38	2610,54	2628,21	2644,43	2659,55	2673,64	2686,54	2698,42	2709.48
55	2453,80	2486,37	2515,03	2540,60	2563,64	2584,65	2603,80	2621,50	2637,86	2653,04	2667,14	2679,98	2691,83	2702 98
60	2447,00	2479,56	2508,27	2533,83	2556,90	2577,95	2597,22	2615,03	2631,37	2646,53	2660,62	2673,55	2685,52	2696,73
65	2440,12	2472,75	2501,53	2526,99	2550,22	2571,24	2590,55	2608,37	2624,75	2640,01	2654,04	2667,07	2679,15	2690,32
70	2433,23	2465,88	2494,56	2520,03	2543,32	2564,47	2583,88	2601,67	2617,96	2633,32	2647,52	2660,63	2672,74	2684,02
75	2426,23	2458,78	2487,62	2513,08	2536,39	2557,61	2576,93	2594,80	2611,19	2626,55	2640,81	2653,99	2666,06	2677,34
80	2419,02	2451,67	2480,45	2506,08	2529,24	2550,50	2569,86	2587,93	2604,29	2619,72	2633,99	2647,12	2659,36	2670,69
85	2411,92	2444,63	2473,33	2499,01	2522,29	2543,54	2563,01	2580,93	2597,45	2613,02	2627,39	2640,60	2652,78	2664.16
90	2404,90	2437,62	2466,32	2491,99	2515,28	2536,63	2556,11	2574,02	2590,60	2606,16	2620,52	2633,74	2646,00	2657,41
95	2397,68	2430,33	2459,13	2484,74	2508,10	2529,46	2549,13	2567,03	2583,65	2599,24	2613,57	2626,94	2639,25	2650,63
100	2390,60	2423,22	2452,13	2477,64	2500,91	2522,30	2541,92	2559,96	2576,56	2592.14	2606,50	2619,75	2632,17	2643.75

Heat.	100 grains of spirit to 70 grains of water.	100 grains of spirit to 75 grains of water.	100 grains of spirit to 80 grains of water.	100 grains of spirit to 85 grains of water.	100 grains of spirit to 90 grains of water.	100 grains of spirit to 95 grains of water.	100 grains of spirit to 100 grains of water.	95 grains of spirit to 100 grains of water.	90 grains of spirit to 100 grains of water.	85 grains of spirit to 100 grains of water.	80 grains of spirit to 100 grains of water.	75 grains of spirit to 100 grains of water.	70 grains of spirit to 100 grains of water.	65 grains of spirit to 100 grains of water.
°	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.
30	2744,20	2753,75	2762 72	2771,08	2778,99	2786,36	2793,22	2799,85	2806,61	2813,85	2821,35	2828,90	2836,39	2844 16
35	2738,13	2747,74	2756 91	2765,32	2773,22	2780,59	2787,54	2794,19	2801,14	2808,52	2816,07	2823,68	2831,36	2839,26
40	2732,24	2741,86	2750,96	2759,50	2767,48	2774,90	2781,84	2788,69	2795,70	2803,17	2810,73	2818,36	2826,31	2834.40
45	2726,09	2735,77	2744,82	2753,36	2761,42	2768,85	2775,94	2782,99	2789,99	2797,45	2805,08	2812,93	2821,00	2829,28
50	2719,93	2729,64	2738,74	2747,27	2755,37	2762,95	2770,14	2777,19	2784,30	2791,72	2799,58	2807,56	2815,71	2824,12
55	2713,60	2723,51	2732,64	2741,24	2749,27	2756,83	2764,09	2771,29	2778,54	2785,96	2793,82	2801,89	2810,23	2818 80
60	2707,40	2717,30	2726,52	2735,17	2743,28	2750,93	2758,17	2765,40	2772,70	2780,26	2788,25	2796,45	2804,85	2813.65
65	2701,05	2710,96	2720,25	2728,98	2737,09	2744,86	2752,21	2759,47	2766,73	2774,43	2782,62	2790,81	2799,38	2808.31
70	2694,76	2704,64	2713,87	2722,75	2730,94	2738,73	2746,06	2753,41	2760,75	2768,45	2776,72	2785,06	2793,80	2802,88
75	2688,14	2698,07	2707,49	2716,35	2724,64	2732,39	2739,89	2747,23	2754,73	2762,58	2770,93	2779,26	2788,00	2797.21
80	2681,50	2691,50	2700,94	2709,76	2718,12	2726,06	2733,53	2740,93	2748,42	2756,43	2764,87	2773,33	2782,14	2791.52
85	2674,95	2684,98	2694,53	2703,33	2711,86	2719,74	2727,25	2734,80	2742,31	2750,22	2758,80	2767,44	2776,33	2785.81
90	2668,29	2678,49	2687,99	2696,91	2705,37	2713,32	2721,01	2728,59	2736,23	2744,24	2752,76	2761,51	2770,59	2780,11
95	2661,51	2671,82	2681,34	2690,33	2698,86	2706,88	2714,61	2722,23	2729,89	2737,98	2746,57	2755,34	2764,57	2774 25
100	2654,76	2664,99	2674,62	2683,63	2692,25	2700,33	2708,04	2715,73	2723,35	2731,55	2740,43	2749,28	2758,48	2768.43

Heat.	60 grains of spirit to 100 grains of water.	55 grains of spirit to 100 grains of water.	50 grains of spirit to 100 grains of water.	45 grains of spirit to 100 grains of water.	40 grains of spirit to 100 grains of water.	35 grains of spirit to 100 grains of water.	30 grains of spirit to 100 grains of water.	25 grains of spirit to 100 grains of water.	20 grains of spirit to 100 grains of water.	15 grains of spirit to 100 grains of water.	10 grains of spirit to 100 grains of water.	5 grains of spirit to 100 grains of water.	Water.	
°	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	
30	2852,03	2859,71	2867,12	2874,43	2881,34	2887,77	2894,22	2900,85	2908,21	2917,19	2928,80	2944,53		
35	2847,45	2855,32	2863,16	2870,87	2878,21	2885,06	2892,07	2899,31	2907,45	2916,95	2928,99	2945,02	2967,14	
40	2842,62	2850,88	2859,06	2867,08	2874,81	2882,30	2889,78	2897,61	2906,39	2916,41	2928,93	2945,25	2967,45	
45	2837,64	2846,16	2854,67	2863,04	2871,22	2879,22	2887,33	2895,67	2904,98	2915,55	2928,49	2945,20	2967,40	
50	2832,76	2841,52	2850,29	2858,96	2867,52	2875,98	2884,57	2893,58	2903,39	2914,42	2927,81	2944,73	2967,05	
55	2827,68	2836,69	2845,72	2854,75	2863,75	2872,67	2881,69	2891,11	2901,42	2913,02	2926,73	2943,98	2966.34	
60	2822,65	2831,90	2841,10	2850,50	2859,87	2869,15	2878,72	2888,62	2899,35	2911,32	2925,50	2942,98	2965,39	
65	2817,49	2826,90	2836,30	2845,97	2855,65	2865,45	2875,49	2885,85	2897,09	2909,43	2923,90	2941,69	2964,11	
70	2812,16	2821,78	2831,61	2841,42	2851,53	2861,63	2872,06	2882,90	2894,56	2907,33	2922,24	2940,13	2962,66	
75	2806,75	2816,63	2826,56	2836,80	2847,14	2857,70	2868,49	2879,67	2891,79	2905,04	2920,17	2938,33	2960,97	
80	2801,25	2811,23	2821,38	2831,92	2842,56	2853,38	2864,54	2876,22	2888,73	2902,35	2917,83	2936,31	2959,07	
85	2795,69	2805,85	2816,32	2827,12	2838,07	2849,28	2860,86	2872,88	2885,56	2899,55	2915,46	2934,14	2956,94	
90	2790,13	2800,40	2811,05	2822,15	2833,38	2844,81	2856,80	2869,16	2882,25	2896,58	2912,84	2931,77	2954,70	
95	2784,36	2794,91	2805,79	2817,08	2828,46	2840,26	2852,47	2865,15	2878,71	2893,44	2910,02	2929,15	2952,08	
100	2778,64	2789,32	2800,25	2811,80	2823,55	2835,30	2848,18	2861,12	2875,07	2890,04	2906,97	2926,28	2949,34	

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TABLE II.

Real specific gravities at the different temperatures.

Heat.	The pure spirit.	100 grains of spirit to 5 grains of water.	100 grains of spirit to 10 grains of water.	100 grains of spirit to 15 grains of water.	100 grains of spirit to 20 grains of water.	100 grains of spirit to 25 grains of water.	100 grains of spirit to 30 grains of water.	100 grains of spirit to 35 grains of water.	100 grains of spirit to 40 grains of water.	100 grains of spirit to 45 grains of water.	100 grains of spirit to 50 grains of water.	100 grains of spirit to 55 grains of water.	100 grains of spirit to 60 grains of water.	100 grains of spirit to 65 grains of water.
30°	,83896	,84995	,85957	,86825	,87585	,88282	,88921	,89511	,90054	,90558	,91023	,91449	,91847	,92217
35	,83672	,84769	,85729	,86587	,87357	,88059	,88701	,89294	,89839	,90345	,90811	,91241	,91640	,92009
40	,83445	,84539	,85507	,86361	,87134	,87838	,88481	,89073	,89617	,90127	,90596	,91026	,91428	,91799
45	,83214	,84310	,85277	,86131	,86907	,87613	,88255	,88849	,89396	,89909	,90380	,90812	,91211	,91584
50	,82977	,84076	,85042	,85902	,86676	,87384	,88030	,88626	,89174	,89684	,90160	,90596	,90997	,91370
55	,82736	,83834	,84802	,85664	,86441	,87150	,87796	,88393	,88945	,89458	,89933	,90367	,90768	,91144
60	,82500	,83599	,84568	,85430	,86208	,86918	,87568	,88169	,88720	,89232	,89707	,90144	,90549	,90927
65	,82262	,83362	,84334	,85193	,85976	,86686	,87337	,87938	,88490	,89006	,89479	,89920	,90328	,90707
70	,82023	,83124	,84092	,84951	,85736	,86451	,87105	,87705	,88254	,88773	,89252	,89695	,90104	,90484
75	,81780	,82878	,83851	,84710	,85493	,86212	,86864	,87466	,88018	,88538	,89018	,89464	,89872	,90252
80	,81530	,82631	,83603	,84467	,85248	,85966	,86623	,87228	,87776	,88301	,88781	,89225	,89639	,90021
85	,81283	,82386	,83355	,84221	,85006	,85723	,86380	,86984	,87541	,88067	,88551	,88998	,89409	,89793
90	,81039	,82142	,83111	,83977	,84762	,85483	,86139	,86743	,87302	,87827	,88312	,88758	,89173	,89558
95	,80788	,81888	,82860	,83724	,84511	,85232	,85896	,86499	,87060	,87586	,88069	,88521	,88937	,89322
100	,80543	,81643	,82618	,83478	,84262	,84984	,85646	,86254	,86813	,87340	,87824	,88271	,88691	,89082
Heat.	100 grains of spirit to 70 grains of water.	100 grains of spirit to 75 grains of water.	100 grains of spirit to 80 grains of water.	100 grains of spirit to 85 grains of water.	100 grains of spirit to 90 grains of water.	100 grains of spirit to 95 grains of water.	100 grains of spirit to 100 grains of water.	95 grains of spirit to 100 grains of water.	90 grains of spirit to 100 grains of water.	85 grains of spirit to 100 grains of water.	80 grains of spirit to 100 grains of water.	75 grains of spirit to 100 grains of water.	70 grains of spirit to 100 grains of water.	65 grains of spirit to 100 grains of water.
30°	,92563	,92889	,93191	,93474	,93741	,93991	,94222	,94447	,94675	,94920	,95173	,95429	,95681	,95944
35	,92355	,92680	,92986	,93274	,93541	,93790	,94025	,94249	,94484	,94734	,94988	,95246	,95502	,95772
40	,92151	,92476	,92783	,93072	,93341	,93592	,93827	,94058	,94295	,94547	,94802	,95060	,95328	,95602
45	,91937	,92264	,92570	,92859	,93131	,93382	,93621	,93860	,94096	,94348	,94605	,94871	,95143	,95423
50	,91723	,92050	,92358	,92647	,92919	,93177	,93419	,93658	,93897	,94149	,94414	,94683	,94958	,95243
55	,91502	,91837	,92145	,92436	,92707	,92963	,93208	,93452	,93696	,93948	,94213	,94486	,94767	,95057
60	,91287	,91622	,91933	,92225	,92499	,92758	,93002	,93247	,93493	,93749	,94018	,94296	,94579	,94876
65	,91066	,91400	,91715	,92010	,92283	,92546	,92794	,93040	,93285	,93546	,93822	,94099	,94388	,94689
70	,90847	,91181	,91493	,91793	,92069	,92333	,92580	,92828	,93076	,93337	,93616	,93898	,94193	,94500
75	,90617	,90952	,91270	,91569	,91849	,92111	,92364	,92613	,92865	,93132	,93413	,93695	,93989	,94301
80	,90385	,90723	,91042	,91340	,91622	,91891	,92142	,92393	,92646	,92917	,93201	,93488	,93785	,94102
85	,90157	,90496	,90818	,91119	,91403	,91670	,91923	,92179	,92432	,92700	,92989	,93282	,93582	,93902
90	,89925	,90270	,90590	,90891	,91177	,91446	,91705	,91962	,92220	,92491	,92779	,93075	,93381	,93703
95	,89688	,90037	,90358	,90662	,90949	,91221	,91481	,91740	,91998	,92272	,92562	,92858	,93170	,93497
100	,89453	,89798	,90123	,90428	,90718	,90992	,91252	,91513	,91769	,92047	,92346	,92646	,92957	,93293
Heat.	60 grains of spirit to 100 grains of water.	55 grains of spirit to 100 grains of water.	50 grains of spirit to 100 grains of water.	45 grains of spirit to 100 grains of water.	40 grains of spirit to 100 grains of water.	35 grains of spirit to 100 grains of water.	30 grains of spirit to 100 grains of water.	25 grains of spirit to 100 grains of water.	20 grains of spirit to 100 grains of water.	15 grains of spirit to 100 grains of water.	10 grains of spirit to 100 grains of water.	5 grains of spirit to 100 grains of water.	Water.	
30°	,96209	,96470	,96719	,96967	,97200	,97418	,97635	,97860	,98108	,98412	,98804	,99334		
35	,96048	,96315	,96579	,96840	,97086	,97319	,97556	,97801	,98076	,98397	,98804	,99344	1,00090	
40	,95879	,96159	,96434	,96706	,96967	,97220	,97472	,97737	,98033	,98373	,98795	,99345	1,00094	
45	,95705	,95993	,96280	,96563	,96840	,97110	,97384	,97666	,97980	,98338	,98774	,99338	1,00086	
50	,95534	,95831	,96126	,96420	,96708	,96995	,97284	,97589	,97920	,98293	,98745	,99316	1,00068	
55	,95357	,95662	,95966	,96272	,96575	,96877	,97181	,97500	,97847	,98239	,98702	,99284	1,00038	
60	,95181	,95493	,95804	,96122	,96437	,96752	,97074	,97409	,97771	,98176	,98654	,99244	1,00000	
65	,95000	,95318	,95635	,95962	,96288	,96620	,96959	,97309	,97688	,98106	,98594	,99194	,99950	
70	,94813	,95139	,95469	,95802	,96143	,96484	,96836	,97203	,97596	,98028	,98527	,99134	,99894	
75	,94623	,94957	,95292	,95638	,95987	,96344	,96708	,97086	,97495	,97943	,98454	,99066	,99830	
80	,94431	,94768	,95111	,95467	,95826	,96192	,96568	,96963	,97385	,97845	,98367	,98991	,99759	
85	,94236	,94579	,94932	,95297	,95667	,96046	,96437	,96843	,97271	,97744	,98281	,98912	,99681	
90	,94042	,94389	,94748	,95123	,95502	,95889	,96293	,96711	,97153	,97637	,98185	,98824	,99598	
95	,93839	,94196	,94563	,94944	,95328	,95727	,96139	,96568	,97025	,97523	,98082	,98729	,99502	
100	,93638	,93999	,94368	,94759	,95152	,95556	,95983	,96424	,96895	,97401	,97969	,98625	,99402	

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When all the experiments had been completed, and the tables here given were just brought into order, an ingenious member of the Royal Society, scarcely less celebrated for his theoretical knowledge than his skill as an artist, published a pamphlet containing censures on our first experiments, and proposing other methods, as much superior to those we had adopted.* In drawing up the Report, in order to avoid prolixity, the reasons for choosing some of the methods were not given, where they did not seem likely to be a subject of controversy; but this pamphlet makes it necessary to assign the motives of our preference, that the public may judge how far we are justified.

First, as to the proportions of the mixtures; which were made by taking an equal quantity of spirit in every instance, and adding to it successively larger quantities of distilled water, as far as to an equal weight; with the intention of going through the watery mixtures on the same plan. This was done for the following reasons: 1. Because it was thought more likely to avoid blunders, if the quantity of only one of the ingredients was changeable, that the operator might not have his attention distracted with computing and weighing out two different quantities for each mixture. 2. Because by this progression the experiments come closer together about the medium degrees of strength, where it was supposed most accuracy would be wanted for practice. 3. As it was thought, from the first, that the best method of adjusting the duty would be by the absolute quantity of alcohol in any mixture, rather than the proportion *per cent.*, or the strength above or under proof, we judged it most expedient not to make the mixtures on either of the two last mentioned principles, lest an undue bias should be given to the judgment, merely from the mode of conducting the experiments. No real difficulty can arise in forming tables of any kind out of these numbers, which answer to an harmonic progression of strength. If the operation be tedious, to obtain the specific gravity of any single proportion, *per cent.* or otherwise, of alcohol and water, the trouble of reducing the whole to a table would not be great, and when once executed, it is done for ever.

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* An account of Experiments to determine the Specific Gravity of Fluids; by J. Ramsden. London, 1792.

Secondly, though the chief reasons for making the mixtures by weight, rather than by measure, have been already assigned in the Report, it is now proper to add something further on that subject. Nothing but arithmetic is required for obtaining the proportions by measure with the utmost exactness; and, as in the former case, though the operation be a little laborious singly, the computation of the entire table will be sufficiently easy. Such a table was recommended in the Report, and can be constructed by any person tolerably conversant with figures. In the pamphlet mentioned above, a method is recommended for proportioning the mixtures by measure, while the actual quantity of spirit is determined by weight, at one operation. The idea is ingenious, but in the execution it seems liable to the following objections. 1. The difficulty of obtaining the full penetration of the spirit and water, in a vessel of the shape required, where, by the intervention of such a narrow neck as is wanted, the free agitation of the fluid must be greatly impeded. 2. The difficulty of getting out all the air-bubbles, produced by the shaking, &c. in a vessel so shaped.

3. The difficulty, or almost impossibility, of bringing the mixture, by the repeated fillings, to coincide exactly with the ring on the neck: for this purpose, the last quantities of water must be put in by such small portions at a time, that scarcely any attention will be equal to the task; and if at length too much be added, it cannot be taken out again without injuring, in some degree, the accuracy of the experiment, which depends on combining the precise quantity of water required to fill the vessel up to the mark, when the full penetration has taken place. 4. In opening the vessel so frequently, to fill it up, a sensible part of the spirit must be lost by evaporation. 5. Moreover it is necessary that, at the end of the operation, the fluid should throughout be exactly of the same temperature as the pure spirit was in the preparatory experiment with it alone; the difficulty of effecting and determining which must be obvious to every one, especially in a vessel of such a size and shape. Lastly, as this vessel is much less manageable than the weighing-bottle, I think the fluid in it cannot be brought to the mark with nearly the same degree of accuracy. These objections, joined to the consideration that no object can be attained in this way, which was not accomplished, with at least equal accuracy, and probably no greater trouble, by weighing the spirit and water separately, determined us not to attempt any experiments with such an instrument.

Thirdly, it is now to be explained why we undertook to determine the effect of heat and cold on the fluids, by means of the weighing-bottle. When, preparatory to our former experiments, that part of the subject came under consideration, the method of ascertaining the expansions and contractions, by means of instruments like thermometers, was one of the first that presented itself. On this occasion, Mr. CAVENDISH was so good as to mention some experiments made by his father, Lord CHARLES CAVENDISH, with instruments on that construction, for the very purpose of determining the expansion of fluids; and other experiments, of the same nature, have appeared in print. The application of this method, however, was thought liable to a most important objection, from the great difficulty there is of being sure that the spirits in the ball are exactly of the temperature indicated by the thermometer placed by the side of it. To enlarge upon this circumstance would be useless, as every person accustomed to experiments is aware, that all possible precautions, joined to the utmost attention in the observer, are scarcely sufficient to ensure this essential correspondence of temperature; which reason alone would have induced us to prefer the method by weight. But there was another argument which still more forcibly determined us in favour of the latter; namely, that the effect of mixture was found in that way, and therefore we were sure it admitted of as great accuracy as was obtained in the other part of the experiments. Greater nicety, if there had been a method which allowed of it, would have been superfluous; and to incur the risk of less accuracy would have been absolutely unjustifiable. By using the same method to determine all the changes of specific gravity, those from heat as well as those from mixture, an uniformity is given to the whole series of experiments, and no one part of the results is liable to more suspicion than another.

Till this time, I believe, the instruments with a ball and tube, for trying expansions, had all been constructed in the manner of real thermometers, to be filled by means of heat;

which circumstance, and the trouble attending it, was a further objection to their use: but in the pamphlet abovementioned are proposed two instruments of this nature, to be filled without heat; one being provided with two equal tubes, the other with a short tube, closed by a stopper. Though both these instruments, and especially the latter, seemed liable to several causes of error, yet, to remove doubts, and bring the method by weight to a proper test, Mr. GILPIN was desired to make some trials with them; Mr. RAMSDEN, the author of the pamphlet, having been previously requested to go through the whole series of experiments on his own plan, which he declined to do. With no small difficulty Mr. GILPIN got the instruments executed; and an account of the experiments to which he subjected them shall be given, in his own words, at the end of this Report. From the perusal of that account, it will be perceived, that the disagreement of the experiments among themselves, is nearly equal to the quantity by which any of them differ from the expansions as obtained by weight. On the whole, however, they give the expansion somewhat less, the cause of which I do not see; possibly it may depend on the fluid in the ball not being quite heated and cooled to the degree shown by the accompanying thermometer; possibly there may be a difference in the expansion of the glass with which the instruments were made, and that of the weighing-bottle, for these numbers are in both cases the excess of the expansion of the fluid over glass; or it may turn on some other circumstance, which has eluded our attention. Whatever may have occasioned the deficiency, I think the experiments will satisfy any one, that most dependence is to be placed on the weights; and at all events the difference is not such as to affect the third place of decimals, or consequently the tables intended for practice.

Probably no one will be surprised that we did not think it necessary to make trial of the weighing-bottle proposed by Mr. Ramsden. Not to mention other inconveniences attending this instrument, it seems evident that a piece of flat glass, with a thermometer projecting from it, laid down on the mouth of a bottle, cannot be depended upon to push off the superfluous liquor equally every time ; and that the proper wiping of the bottle, when so covered, will be attended with difficulties of various kinds.

It is true that the experiments by weight took up much time, and demanded great patience. But I believe that similar experiments, by the methods recommended in the pamphlet, if executed with the same degree of accuracy, would be found not much less tedious. However this may be, it is a consideration of no consequence, provided the results at length obtained be right. Now of these there is no direct impeachment, though some doubts are thrown upon them, on four accounts ; evaporation ; condensation of moisture on the weighing-bottle ; difficulty of shaking the fluid in it ; and uncertainty in determining the heat. With regard to evaporation, its effect, we hope, has been ascertained, and allowed for, in these new experiments. All error from condensation of moisture was obviated by careful wiping. The fluid in the weighing-bottle was agitated, and mixed together, by means of the thermometer immersed in it ; besides which, a considerable degree of motion could be given to it, even when the ball was very nearly full, by shaking the bottle in various directions. Mr. Gilpin's known accuracy, and the care he bestowed on these experiments, must gain him credit for having duly watched the thermometer, so as to seize the moment when it gave the just temperature of the mass.

Our experiments were finished, and the tables now given were drawn out, before the appearance of Mr. RAMSDEN's pamphlet. Yet if any of the methods he proposed had been really preferable, the whole series should have been repeated on that new plan, and particularly with regard to the effect of heat, if the instruments for that purpose had been found to answer the character given of them. But as this was not the case, we have thought it right to adhere to an obvious and direct method, in which, however laborious, there can be no fallacy, and the uniformity of which ensures an equal degree of accuracy to every part of the operation.

SINCE the publication of our first tables, several hydrometers have been contrived, with the view of applying them to practice. Those of copper were rejected on account of the errors which small and almost imperceptible bruises in them might occasion; and for the same reason no other metal was tried. Mr. GILPIN has constructed two areometers of glass; one with the stem so divided, that an easy table may be formed for the correction necessary according to the different weights with which it is used; the other with a separate scale fixed to each of those weights, made to slip into the tubular stem of the instrument; a contrivance that obviates the necessity of a table. Mr. RAMSDEN also has invented a balance hydrometer, with several varieties of construction, one of which is detailed in his pamphlet. All the above-mentioned instruments appear to have fully as much accuracy as can be required; of their preference in other respects, the practical officers who are to use them will probably be found the best judges. That which can be managed with the greatest facility and quickness, which affords the least opportunity of making blunders, which is least liable to be out of order, and shews most immediately if it be so, will unquestionably prove the most satisfactory in practice. Hydrometers having a thermometer inclosed within them must be condemned, as not ascertaining the temperature with the requisite precision. An attempt to supersede the use of the thermometer, by employing for the hydrometer, a substance which "has the same "degree of expansion as the mean of the compounds," is very inconsistent with the kind of accuracy sought by these experiments.

As an allowance is made, in our table of specific gravities, for the expansion and contraction of the glass weighing-bottle, this must be taken into the account, with every areometer, whenever much exactness is desired.

I am still of opinion, that the best way of laying the duty would be directly on the quantity of alcohol contained in any composition; and though this might require too great a change in the present system of laws, yet as the same principle may be applied in estimating the strength, and taking stock, I will just mention in what manner the computation can be most readily made. From the numbers in this Supplementary Report a table must be constructed, on the top of which shall stand every degree of heat from 40, or 30, to 80, and at the side every specific gravity from ,825 to 1,000, if it be thought necessary, or as much less as will answer the purpose. The places of this table are to be filled up, by computing, from the original tables, the quantity by measure of alcohol and water corresponding to each specific gravity and degree of heat; and then dividing the quantity of alcohol by the whole quantity of the mixture; thus a decimal multiplier will be obtained, which must be put in the compartment of the table formed by the intersection of the columns of that particular heat and specific gravity. When the table is completed in this manner, we have only to multiply the contents of any cask, as found on gauging it, by the decimal number given in the table for the heat and specific gravity of the liquor, and the product will be the quantity of pure alcohol it contains. Hence it must be evident, that no objection can lie to this method on account of difficulty; if, however, it be thought more eligible, for different reasons, to adopt the proportion of alcohol *per cent.*, the relation of strength to the point of proof, or any other method, the numbers in this Report will equally apply to all, with the proper variation in the table to be employed.

As to the calculation necessary for constructing the table of decimal multipliers, what has been already said with respect to the reduction of the harmonic numbers applies also to it. The labour of the whole will not be very great, and it is once for all. The process is not an approximation, but a plain arithmetical computation, which may be carried on true to as many decimal places as the experiments will allow. For this purpose, indeed, it is necessary to have the weight of a known measure of water. Mr. RAMSDEN's method of obtaining this, by means of a cylinder, is far preferable to that of hollow cubes, particularly if the ends of the cylinder can be made as true as the body of it. But in applying this instrument to fix the term of proof, as proposed by that gentleman, it must be remembered, that 7lb. 13 oz. is not the weight of a gallon of proof spirit, but of spirit one to six under proof. On that proportion the value of proof was computed in the Report, by the same rule as Mr. RAMSDEN has since given, but which it was not thought necessary to detail at full length.

Although, as was said in the original Report, the quantity of extraneous substances usually found in spirituous liquors does not increase their specific gravity so much as to be worth the consideration of Government, yet this is by no means the case when such substances are added intentionally. The effect of alkalies is well known. Mr. RAMSDEN'S experiments shew how great a change of specific gravity is produced by sugar, when dissolved plentifully in weak liquors; and in an experiment made by order of the Board of Excise, $\frac{1}{64}$ part of sugar, put into very strong spirit, reduced its apparent strength no less than 17 *per cent.* by CLARKE'S hydrometer.

I conclude with observing, that the *execution* of the experiments, and of the computations, rested entirely with Mr. GILPIN, who is responsible for their accuracy, and entitled to the praise they may be found to merit. For the general plan, as well as the particular methods adopted, I hold myself accountable, and have now so fully stated my reasons for what I recommended to be done, that any competent person will readily judge of their validity. In this and the foregoing Report, I have purposely avoided all philosophical deductions, and a comparison with former experiments; that the narrative might not be loaded with any foreign matter, to interfere with the practical object for which this business was undertaken.

C. BLAGDEN.

Appendix to the foregoing Report. In a Letter from Mr. George Gilpin, *Clk. R. S. to* Charles Blagden, *M. D. Sec. R. S.*

SIR,

HAVING completed two instruments for trying the expansion of fluids, according to the method described by Mr. RAMSDEN, with a stopper going into a tube on the side of the ball, I now present you with an account of the experiments which I have made with them, that you may judge how far such instruments are deserving of notice. The scale of the longest admits of ,26 of an inch for each degree of the thermometer, and that of the shortest ,17 of an inch for each degree.

They were charged with pure spirit: some of the same that was made use of in our experiments by weight, specific gravity ,82514: and having hung them up by the side of each other to a piece of wood, provided for the purpose, with the same sensible thermometer hanging between them that was used in our experiments by weight, I immersed them in a large quantity of water brought to the temperature of 60°, the one quite, the other nearly, to the height of the fluid in the stem. In this water they were suffered to continue until they had arrived at that temperature, when it was observed that the spirit in the tube of the long instrument stood at 0, or the commencement of the scale, and the spirit in the tube of the short instrument stood at $\frac{15}{10000}$ above 0, which I shall in future for shortness call 1,5, and which it is evident must be applied with the sign + or — to the quantity of expansion or contraction read off from above or below 0, as the case may require. They were then cooled down to 30° of temperature, when the spirit in the long instrument was found to stand 165, and that in the short instrument 163,5 below 0. They were again brought to the temperature of 60°, when the spirit in the long instrument was found to stand 0,5, below 0, and in the short instrument 1,5 above 0 as before. They were then heated to 100°, when the spirit in the long instrument stood at 231, and in the short instrument at 233,5 above 0. I brought them again to the temperature of 60°, when the spirit in the long instrument was found to stand 3 below 0, but in the short instrument 1,5 above 0 as before.

It appears from these experiments, that the contraction of the spirit, by the long instrument, for 30 degrees, that is, in cooling down from 60° to 30°, is 165; but in heating it up again to 60°, it was found not to stand at 0, as before, but 0,5 below; therefore the expansion will be only 164,5, the mean is 164,75. The expansion, on heating up from 60° to 100°, will be $231+0,5=231,5$; but on cooling down from 100° to 60° again, the spirit was found to sink 3 below 0, therefore it will be $231+3=234$; the mean 232,75, and the total expansion, from 30° to 100°$=397,5$; differing from ours, in defect, by 0,05 of a division. But the two methods of heating from 60° to 100°, and cooling from 100° to 60° again, differ 2,5 divisions, or so many ten thousandth parts of the whole.

The contraction from 60° to 30°, by the short instrument, appears to be $163,5+1,5=165$, and the expansion, on heating up again to 60°, the same; from 60° to 100° it was found to be 233,5 — 1,5 = 232, and the contraction in cooling down again from 100° to 60° the same; the total expansion 397, differing from ours 0,55 of a division, in defect.

After the above experiments, the instruments were emptied of the spirit; and another day, preparatory to a repetition of the experiments, they were charged again with some of the same spirit that was used before, and the results found to be as follow.

Having brought them to the temperature of 60°, I found the spirit in the long instrument to stand 3 above 0, and in the short instrument 5 below 0. They were then cooled down to 30° of temperature; when the spirit in the long instrument was found to sink to 161,5, and in the short instrument to 167,5. They were afterwards brought back again to 60° of temperature; when the spirit in the long instrument stood 3 above 0, as before, but in the short instrument 5,5 below 0. I then heated them up to 100°, and it stood in the long instrument at 234, and in the short instrument at 226, above 0. They were again brought to the temperature of 60°; when it was found to stand in the long instrument at 0, and in the short instrument at 8 below 0.

From the above experiments it appears, that the contraction by the long instrument, in cooling down from 60° to 30°, is 161,5 + 3 = 164,5, and the expansion in heating up again to 60°, the same. In heating up from 60° to 100°, 234 — 3 = 231; but the contraction in cooling down again from 100° to 60°, 234; the mean is 232,5, and the total expansion from 30° to 100° = 397, differing from the experiments by weight 0,55 of a division, in defect: but if no mean be taken, the deficiency will appear greater. The difference between heating up from 60° to 100°, and cooling down again from 100° to 60°, is 3 divisions, still more considerable in this than in the last experiment.

The contraction by the short instrument from 60° to 30° is 167,5 − 5 = 162,5, and the expansion from 30° to 60° again 167,5 − 5,5 = 162; the mean is 162,25. On heating up from 60° to 100°, 226 + 5,5 = 231,5; but the contraction, in cooling down again from 100° to 60° was 226 + 8 = 234; the mean is 232,75, and the total expansion from 30° to 100° = 395; differing from the experiments by weight 2,55 divisions, in defect. The difference between heating up from 60° to 100°, and cooling down again from 100° to 60°, is 2,5 divisions.

After the above experiments had been made, the spirit was let out, and, on a subsequent day, the two instruments were charged again with some of the same spirit, previous to the following experiments.

After bringing the spirit to the temperature of 60°, I found it to stand in the long instrument 6 above 0, and in the short instrument 2 below 0. I cooled the spirit down to 30°, when it stood in the long instrument 158,5, and in the short instrument 166, below 0. I brought it again to the temperature of 60°, and it returned to the same point it set off from, in both instruments. The spirit was then heated to 100°; when it rose in the long instrument to 235, and in the short instrument to 230, above 0. I cooled it again to the temperature of 60°, when it was found to stand in the long instrument 1 below 0, and in the short instrument 5 below 0.

It appears, from the above experiments, that the contraction by the long instrument in cooling down from 60° to 30° is 158,5

+6 = 164,5, and the expansion in heating up from 30° to 60°, the same. On heating up to 100°, 235—6 = 229, but the contraction in cooling down from 100° to 60° again, 235+1= 236; the mean is 232,5, and the total expansion from 30° to 100° = 397,0; differing from the experiments by weight 0,55 of a division, in defect; but the two methods in this experiment differ very considerably from each other, namely, by no less a quantity than 7 divisions. In this experiment it seems probable either that some of the spirit leaked out at the stopper, or that the stopper shifted its place a little, so as to enlarge the capacity of the ball.

The contraction by the short instrument in cooling down from 60° to 30° was 166 — 2 = 164, and the expansion on heating up again to 60°, the same. On heating up to 100°, it was 230 + 2 = 232, but on cooling down again to 60° the contraction was 230 +5 = 235; the mean is 233,5, and the total expansion 397,5; differing from the experiments by weight 0,05 of a division, in defect. The difference between the two methods, in heating up from 60°, to 100°, and in cooling down again from 100° to 60°, is three divisions.

It appears from the preceding experiments, that the mean of all the quantities found on heating up from 30° to 100°, and cooling down from 100° to 30°, taken together, gives for the total expansion 397,16 by the long instrument, and 396,5 by the short; the former errs 0,39, and the latter 1,05 divisions from the experiments by weight, in defect. It appears also that the mean of all the quantities found by the long instrument, on heating up from 30° to 100°, gives for the total expansion 4,34 divisions less than the mean of all the quantities taken together, by the same instrument, on cooling down from

100° to 30°; and the difference by the short instrument is 2 divisions.

The following experiments were made with a mixture of equal parts of spirit and water by weight; the spirit being of the strength already mentioned.

Having charged the instruments with the mixture, and brought it to the temperature of 60°, it was found to stand in both of them at 1 above 0. The mixture was then cooled down to 30°, when it stood at 125 below 0 in the long instrument, and 124,5 in the short instrument. It was brought back to the temperature of 60°, when, in the long instrument it was found to stand 1,5 above 0, but in the short instrument 1 above 0 as before. I heated the mixture to 100°, when it stood at 185 in the long instrument, and in the short instrument at 183,5 above 0. The mixture was afterwards cooled to the temperature of 60°, when it was found to stand 2,5 above 0 in the long instrument, but in the short instrument 1,5 below 0. After keeping them upright in the temperature of 60° two hours, I found the mixture in the long instrument to stand 3 above 0, but in the short instrument 2 below 0. I heated it again to 100°, when the mixture in the long instrument was found to stand 185, and in the short instrument 180,5 above 0. I brought the mixture again to the temperature of 60°, and found it stand 2,5 above 0 in the long instrument, and in the short instrument 4 below 0.

From the above experiments the contraction of this mixture from 60° to 30° was found to be, by the long instrument, $125 + 1 = 126$; but in heating up to 60° again, the expansion was $125 + 1{,}5 = 126{,}5$; the mean is 126,25. The expansion in heating up from 60° to 100° was $185 - 1{,}5 = 183{,}5$, but the contraction in cooling down from 100° to 63° was 185 — 2,5 = 182,5. In heating up again to 100° it was 185 — 3 = 182, but in cooling down again to 60°, 185 — 2,5 = 182,5; the mean of the four is 182,62, and the total expansion from 30° to 100° = 308,87; differing from the experiments by weight 1,7 division, in defect. The difference between the two methods of heating up from 60° to 100°, and cooling down again from 100° to 60°, taking a mean of the two heatings, and the mean of the two coolings, is 0,75 of a division.

The contraction by the small instrument, in cooling down from 60° to 30°, was 124,5 + 1 = 125,5. On heating up again to 60°, the expansion was the same. In heating up from 60° to 100° the expansion was 183,5 — 1 = 182,5; but in cooling down to 60° again, the contraction was 183,5 + 1,5 = 185. In heating again up to 100°, the expansion was 180,5 + 2 = 182,5; but in cooling again to 60° the contraction was 180,5 + 4 = 184,5. The mean of these four gives 183,62, for the expansion from 60° to 100°; and therefore the total expansion from 30° to 100° will be 309,12, differing from the expansion found by the experiments by weight 1,45 division, in defect. The difference between the mean of the two heatings up from 60° to 100°, and the two coolings down from 100° to 60° again, is 2,75 divisions.

The mixture made use of in the above experiment was now emptied out, and the instruments were charged with more of the same, preparatory to the following experiments.

The mixture being brought to the temperature of 60°, was found to stand in each of the instruments at 1,5 above 0. It was then cooled down to 30°, and it stood in the long instrument 124,5, and in the short instrument 125, below o. It was then brought back to the temperature of 60°, and it stood in the long instrument 1,5 above o as before, but in the short instrument 1 above o only. I heated the mixture to 100°, when it stood at 182,5 in the long instrument, and in the short instrument at 183,5 above o. The mixture was afterwards brought to the temperature of 60°, when it was found to stand at o in both instruments.

It appears from the preceding experiments, that the contraction of this mixture in cooling down from 60° to 30°, by the long instrument is 124,5 + 1,5 = 126, and the expansion in heating up again from 30° to 60°, the same. The expansion in heating up from 60° to 100° was 182,5 — 1,5 = 181; but in cooling down again from 100° to 60°, the contraction was 182,5; the mean is 181,75, and the total expansion from 30° to 100° will be 307,75, differing from the experiments by weight 2,82 divisions, in defect. The difference between the two methods of heating up from 60° to 100°, and cooling down again from 100° to 60°, is 1,5 division.

The contraction of the mixture in cooling down from 60° to 30°, by the short instrument is 125 + 1,5 = 126,5; but in heating up again to 60°, the expansion was 125 + 1 = 126; the mean is 126,25. The expansion in heating up from 60° to 100° was 183,5 — 1 = 182,5, but in cooling down again from 100° to 60° the contraction was 183,5; the mean is 183; and therefore the total expansion from 30° to 100° will be 309,25, differing from the experiments by weight 1,32 division, in defect. The difference between the two methods of heating up from 60° to 100°, and cooling down again from 100° to 60°, is 1 division.

It appears from all the preceding experiments with the mixture of equal parts of spirit and water, that the mean of all the quantities found on heating up from 30° to 100°, and cooling down again from 100° to 30°, taken together is, for the total expansion, 308,46 by the long instrument, and by the short instrument 309,29; the former errs 2,11, and the latter 1,28 divisions, in defect, from the experiments by weight; and that the mean of all the quantities found by the long instrument, in heating up from 30° to 100°, gives for the total expansion 0,33 division less than the mean of all the quantities taken together, by the same instrument, in cooling down from 100° to 30°. The difference by the short instrument is 1,83 division.

Although the results found from the preceding experiments come nearer those of the experiments by weight than might have been expected, considering the many objections that instruments of this kind must naturally present, and the great differences which were actually found among themselves on repeating the experiments, especially in the expansion of pure spirit, where the difference has been equivalent to 1,68 grains in weight, upon the quantity used in our experiments with the weighing-bottle; yet I think, after a careful perusal of the foregoing facts, I shall not be thought too precipitate when I say, that these instruments neither do nor can possess that accuracy which we have been led to expect from them. We have seen, in the foregoing experiments, that there has sometimes been apparently a loss of some part of the fluid, after an alteration of the temperature; at other times there appeared to be no loss at all; and sometimes there appeared to be even more spirit in the instrument than there was at first. These contradictory facts may, I apprehend, be accounted for in the following manner. The mechanical operation of grinding a stopper that will fit so delicate a tube, as is here necessary, perfectly tight, must be acknowledged to be difficult; and should it happen to be done accurately, so that none of the fluid is lost in one degree of temperature, it is very doubtful whether, on exposing this instrument to a different temperature, the expansion would be the same in both the tube and stopper. It appears most probable, from these experiments, that they actually did not expand alike, as perhaps no two pieces of glass ever do; and the effect to be expected from a less expansion of the stopper than of the tube is, either that some of the fluid would leak out, or that the capacity of the ball would be enlarged. But the chief reason why there may sometimes seem to be a loss, at other times no loss at all, I apprehend to be, that more of the fluid will adhere to the upper part of the tube, on filling it, at one time than at another. In the use of this instrument also, a small error may arise from the stopper not being always put in exactly alike; in which case the capacity of the instrument would be altered; and, of course, the divisions on its stem would not give the expansion of the fluid accurately. Care was always taken in these experiments to put the stopper in as nearly alike as possible; but it might not perhaps always be done exactly so.

It is also obvious, that experiments with this instrument will be affected by another source of error, if made in the manner which is recommended, namely, by heating the fluid up from 60° to 100°, and cooling it down again to 30°: for it must be evident that the whole length of the tube will then be left wet by the fluid, in sinking from 100° to 30°, and consequently the expansion will be made to appear too great. The effect of this circumstance will be very considerable; but in the use of this instrument we have no certain means of ascertaining with accuracy the quantity of error occasioned by it, because that quantity falls in with other errors. Of this I shall speak more hereafter, in my remarks on another instrument which I constructed, without a stopper, and which was also recommended by the same gentleman.

THE former experiments were all made with that kind of instrument which has a tube rising from the side of the ball, with a ground-glass stopper inserted into it; an instrument we have seen by no means to be considered as sufficiently accurate for ascertaining the expansion of fluids; I therefore constructed another, similar to the other of the two recommended by Mr. RAMSDEN, which has two tubes rising from the ball, one on each side.

Having charged this instrument with some of the same spirit which was made use of in the former experiments, and brought it to the temperature of 60°, the spirit in the two tubes was found to stand at 4 above 0. It was then cooled down to 30°, when the spirit in the two tubes was found to stand at 161 below 0, the instrument being always so held as to bring it to the same point in both tubes. I then heated it up to 100°, and it stood in the two tubes at 236 above 0. I cooled it again to 30°, when it was found to stand in the two tubes at 162 below 0. It was again heated up to 100°, and it stood in the two tubes at 236 above 0. I then brought it again to the temperature of 60°, and found it to stand in the two tubes at no more than 3 above 0.

It appears from the above experiments, that the contraction of the spirit from 60° to 30° is 161 + 4 = 165, and the expansion in heating up again to 60°, the same. On heating up from 60° to 100°, 236 — 4 = 232, and therefore the total expansion from 30° to 100° is 397; but in cooling down from 100° to 30°, the total expansion will be 236 + 162 = 398; the former quantity differs 0,55, in defect, and the latter 0,45 of a division, in excess, from the experiments by weight. Now it is evident that the method of heating up from 30° to 100° can only be admitted as giving a true result, for it was found on cooling the spirit down from 100° to 30°, that the contraction from 60° to 30° had been increased by 1; so much of the fluid being left behind in the upper part of the two tubes, as appears on its being heated up again to 60°, for then it stood lower in the two tubes by 1 than it did before; care having been taken that the upper part of the two tubes should be as dry as possible before the experiment commenced, for which purpose the instrument was charged over night, and constantly kept in a vertical position. It must also be obvious that 397, which was found for the total expansion in heating up from 30° to 100°, must likewise be too great by nearly the same quantity, it having been cooled down from 60° to 30° previous to its being heated up to 100°, as this would tend to make it sink so much lower than it would have done in the first instance had that not been the case.

After the above experiments, the spirit was poured out, and, previous to the repetition of the foregoing experiments on a future day, it was charged with more of the same spirit which was used in the former experiments, and the instrument was hung up as before.

Having brought it to the temperature of 60°, the spirit in the two tubes was found to stand at 4 above 0. I cooled it down to 30°, when it stood in the two tubes at 160,5 below 0. I brought it again to the temperature of 60°, and it was found to stand in the two tubes at 4 above 0 as before. It was then heated up to 100°, and was found to stand in the two tubes at 236 above 0. I cooled it down again to 30°, and found it to stand in the two tubes at 162 below 0. It was then brought again to the temperature of 60°, and was found to stand in the two tubes at no more than 2,5 above 0.

From the preceding experiments it appears, that the contraction in cooling down from 60° to 30°, is $160,5 + 4 = 164,5$, and in heating up from 30° to 60° again, the expansion was the same. In heating up from 60° to 100°, the expansion was $236 - 4 = 232$, therefore the total expansion in heating up from 30° to 100° will be 396,5; but in cooling down again from 100° to 30°, we shall have for the total expansion $236 + 162 = 398$. The former quantity of 396,5 differs 1,05 in defect, and the latter 0,45 of a division in excess, from the experiments by weight: but it is obvious from this, as well as from the preceding experiment, that the method of heating up from 30° to 100° can only give the true expansion, as has already been observed: for when the spirit is cooled down from 60° to 30° the expansion will be made greater than it ought to be; as it was found on setting off, that the spirit in the two tubes at 60° of temperature stood at 4 above 0; and after having been cooled down to 30°, and heated up again to 60°, it was found to stand the same: but after having been heated up to 100°, and cooled down again to 30°, the contraction from 60° to 30° was found greater by 1,5 than before; and on heating up to 60° again, it was found to stand only 2,5, instead of 4, above 0. It is therefore very reasonable to conclude, that a quantity equal to 1,5 had adhered to the upper part of the tubes, and no well-grounded objection can be made to this, when we consider that 1 division is only equal to ,015 of a grain of spirit, in this instrument.

It appears then from the preceding experiments, that the mean of the quantities found, on heating up from 30° to 100°, including the error that must arise from some of the fluid adhering to the tube, in cooling it down from 60° to 30°, previous to its being heated up from 30° to 100°, gives for the total expansion of the spirit 396,75; and in cooling down from 100° to 30°, 398,0; the difference is 1,25: but I have already shewn that this difference is not so great as it would have been had it not been cooled down from 60° to 30°: if therefore we say, as 232 : 1,25 : : 164 : 0,88, it is evident that the latter quantity must be subtracted from 396,75, and there will remain for the total expansion of the spirit by this instrument, in heating up from 30° to 100°, 395,87, which is different from the experiments by weight 1,68 division, in defect.

The following are experiments made with the same instrument, and a mixture of equal parts of spirit and water, being some of the same which was used in the former experiments.

Having charged the instrument with this mixture, it was immediately put into a vessel of water, whose temperature was 60°, and the mixture was found to stand in the two tubes at 3,5 above 0. I then cooled it down to 30°, and it sunk to 122 below 0. I heated it up to 100°, when it was found to rise to 188 above 0. It was afterwards brought to the temperature of 60°, and suffered to remain in that temperature for three hours, when it was found to stand at 6 above 0.

It appears from the above experiments, that the contraction in cooling down from 60° to 30°, is 122 + 3,5 = 125,5; and on heating it up to 100°, we have for the total expansion from 30° to 100°, 122 + 188 = 310; but it is obvious that this total expansion cannot be the true one; for it appears, on suffering the instrument to remain three hours in the temperature of 60°, that it was found to have collected a quantity = 2,5, that had undoubtedly adhered to the upper part of the tube when charged, and the fluid having arrived at the temperature of 60° sooner than what adhered could descend, it was of course left behind on cooling the mixture down to 30°; if therefore that quantity had been collected while it remained at the temperature of 60°, it would have stood at 6 above 0, and the contraction from 60° to 30° would have been 119,5 + 6 = 125,5 below 0; and the total expansion from 30° to 100°, = 119,5 + 188 = 307,5 instead of 310, as was found before; and differing from the experiments by weight 3,07 divisions, in defect.

The same mixture having been suffered to remain in the instrument, which was hung up as before, the following experiments were tried two days afterwards.

Having brought the mixture to the temperature of 60°, it was found to stand in the two tubes at 5,5 above 0. It was cooled down to 30°, and was found to sink to 120 below 0. I then heated it up to 60°, and found the mixture in the two tubes to stand at 5,5 above 0 as before. It was afterwards heated up to 100°, when the mixture in the two tubes was found to have risen to 187 above 0. I again cooled it down to 30°, and found it to stand in the two tubes at 122 below 0. Lastly, I heated it again up to 60°, and found it to stand in the two tubes at no more than 4 above 0.

From the above experiments it appears, that the contraction of the mixture in cooling down from 60° to 30°, is 120 + 5,5 = 125,5; and the total expansion on heating up from 30° to 100°, including the error arising from cooling it down from 60° to 30°, will be 120 + 187 = 307; but in cooling down again from 100° to 30°, we shall have for the contraction 187 + 122 = 309. The former quantity of 307 errs from the experiments by weight 3,57, and the latter 1,57 division, in defect.

But by taking a mean of the quantities found on heating the mixture up from 30° to 100°, including the error arising from some of the fluid being left adhering to the tube, in cooling down from 60° to 30° previous to its being heated up from 30° to 100°, we shall have for the total expansion 307,25, and it was found in cooling down from 100° to 30° to be 309; the difference is 1,75; if then we say, as 182 : 1,75 : : 125,5 : 1,21, the last number being subtracted from 307,25, we shall have for the true expansion in heating up from 30° to 100°, 306,04; differing from the experiments by weight 4,53 divisions, in defect.

From what was advanced by Mr. RAMSDEN respecting the accuracy of the two instruments with which the foregoing experiments have been made, there was great reason to expect that different results would have been found. It appears that no dependance ought to be placed on experiments made with that kind of instrument which has a tube rising from the side of the ball, to be closed with a stopper. More accuracy may undoubtedly be expected from experiments made with the other kind of instrument which has two tubes, because one of the inconveniences attending the former is removed in it: but we have seen that even experiments made with this instrument do not bear the same marks of accuracy as the experiments by weight; nor can this be much wondered at, if it be considered that the trifling error of ,027 of an inch, in constructing the instrument, will produce an error of one division, which is equal to 0,24 of a grain on the quantity contained in our weighing-bottle; and how difficult and uncertain it is in such an instrument to ascertain the exact temperature, by placing a thermometer only by the side of it. It is not uncommon to see the fluid in the expansion instrument, and the mercury in the thermometer, move contrary ways: and I have more than once observed an alteration in the thermometer, of more than half a degree, when no alteration whatever has been produced in the fluid in the expansion instrument. Indeed, on the least reflection it must be obvious to every one, that the changes of temperature of the fluid in a ball of $1\frac{1}{4}$ inch diameter, cannot be expected to be so quick as in one of 0,22 of an inch. It is also of the utmost consequence in making experiments with this instrument, though it will render it extremely tiresome to the experimenter, that it be in continual motion; for should that precaution not be observed, very considerable errors indeed will take place.

I am, &c.

Apartments of the Royal Society,
Somerset Place.

G. GILPIN.

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